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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIII. *Nov.* 12, 1852. No. 1.

J. C. ADAMS, Esq., President, in the Chair.

Rev. W. C. Williams, North London Collegiate School; and Richard Lee, Esq., Wheatley, Oxon,

were balloted for and duly elected Fellows of the Society.

DISCOVERY OF THALIA.

On the evening of December 15th, at $6^h\ 30^m$ mean time, Mr. Hind detected, at Mr. Bishop's Observatory, another small planet, belonging to the group between *Mars* and *Jupiter*. Like *Calliope*, this planet has been brought to light by means of the ecliptical star-maps, which are now in course of publication from the above Observatory. The following are the early observations of the planet, which has been named *Thalia* by Mr. Bishop. It shines as a star of between the tenth and eleventh magnitudes, with a pale watery light.

1852.	Green. M.T. h m s	App. R.A. h m s	App. N.P.D. ° ′ ″
Dec. 15	7 18 39	3 12 4·98	73 10 16·4
	8 42 2	2·70	3·9
17	6 2 6	3 10 48·23	73 3 26·0
20	6 17 14	3 9 4·87	72 52 3·9

LUTETIA.

Elements.

By M. George Rümker, from the Observations at Paris, Nov. 18; and at Hamburg, Nov. 28 and Dec. 3.

M	= 61 29 23·3	1852, Dec. 0, Berlin M.T.
π	309 53 19·4	1853, Jan. 0, Mean Equinox
Ω	78 38 48·4	
I	3 19 49·7	
φ	19 51 55·2	
Log a	= 0·4157699	log μ = 2·9263517

An ephemeris, from Dec. 6 to Jan. 3, was sent at the same time, which has been communicated to the principal observers.

MASSILIA.

Elements. By M. G. Rümker.

Computed from the Hamburg Observations, Sept. 30, Oct. 26, and Nov. 19.

M	.....	297° 0′ 46″·0 Nov. 0·0, 1852, Berlin M.T.
π	.....	94 32 39·7 } Mean Eq^{x}.
Ω	.....	207 8 47·6 } Jan. 0, 1853.
i	.....	0 40 28·1
φ	.....	10 3 27·5
Log a	.....	0·3890493
Log μ	.....	0·2664326

These elements represent the middle observation, thus:

Computation – Observation } +0″·4 in Longitude, +0″·0 in Latitude

An ephemeris based on these elements was sent by M. G. Rümker at the same time: this has been forwarded to our principal observers.

The observations of the solar eclipse of July 28, 1851, which form Part I. of Vol. XXI. of the *Memoirs*, are now published. There are six plates and 120 pages of letterpress. The price to Fellows is 5*s*., and 10*s*. to the public. There is a separate title-page for the purchasers of the part who may not want the volume.

On a New Method of Computing the Perturbations of Planets. By M. Encke, Director of the Royal Observatory of Berlin. Translated into English and Illustrated with Notes by G. B. Airy, Esq., Astronomer Royal. (Appendix to the *Nautical Almanac* for 1856.)

The main object of the author is to supply an easy and accurate method of computing the perturbations of the various small planets revolving between *Mars* and *Jupiter*, the number of which is constantly increasing from fresh discoveries. The method, however, is applicable to any of the other bodies of the planetary system.

Let t denote the time from which it is proposed to compute the action of the disturbing body. By a slight transformation the differential equations of the motion of the disturbed planet are reduced to the following form:—

$$\xi = \iint \left(X + \frac{k^2}{r^{\circ 3}} \left(3 \frac{x^\circ}{r^\circ} \delta r - \xi \right) \right) dt^2$$

$$\eta = \iint \left(Y + \frac{k^2}{r^{\circ 3}} \left(3 \frac{y^\circ}{r^\circ} \delta r - \eta \right) \right) dt^2$$

$$\zeta = \iint \left(Z + \frac{k^2}{r^{\circ 3}} \left(3 \frac{z^\circ}{r^\circ} \delta r - \zeta \right) \right) dt^2,$$

x°, y°, z°, being the elliptical co-ordinates of the disturbed planet, and r° its radius vector, as deducible from the elements of the orbit in which it was revolving at the instant t; X, Y, Z, the resolved values of the disturbing force in the directions of corresponding rectangular axes; ξ, η, ζ, the perturbations of x, y, z, counted from t; δr, the perturbation of the radius vector; and k a constant quantity.

These equations are integrated by mechanical quadratures, the successive numerical values of the quantity under the sign of the double integral in each equation being corrected by a peculiar process, founded on the theory of finite differences.

The first step is to divide the period for which the perturbations are to be computed, into a number of sufficiently small intervals of time, each equal to τ, and to calculate the values of X, Y, Z, for the times $t - \frac{1}{2}\tau$, $t + \frac{1}{2}\tau$, $t + \frac{3}{2}\tau$, &c., using for this purpose the elliptical co-ordinates x°, y°, z°, of the disturbed planet. As ξ, η, ζ, $\frac{d\xi}{dt}$, $\frac{d\eta}{dt}$, $\frac{d\zeta}{dt}$, are assumed severally equal to zero at the commencement of the period for which the perturbations are to be computed, the first three values of the quantities under the sign of the double integral (or $\frac{d^2\xi}{dt^2}$, $\frac{d^2\eta}{dt^2}$, $\frac{d^2\zeta}{dt^2}$), are readily found with sufficient accuracy by an approximative process, and from them the corresponding values of ξ, η, ζ, are deduced. For all the subsequent values of ξ, η, ζ, the computation is uniform. The mode of procedure is this:—From the three last values of $\frac{d^2\xi}{dt^2}$ an approximate value of the same quantity for the following argument is readily deduced, and a provisional value of ξ is then found by means of the formula of integration $\xi = "f + \frac{1}{12} \cdot \frac{d^2\xi}{dt^2}$, $"f$ being a numerical quantity computed with the utmost facility by the aid of results already obtained. The corresponding values of η and ζ being found by a similar process, the three resulting numbers are employed in computing the second part of the expression under the integral sign in each of the fundamental equations. Finally, the numbers thus found, when applied to X, Y, Z, give the corrected values of $\frac{d^2\xi}{dt^2}$, $\frac{d^2\eta}{dt^2}$, $\frac{d^2\zeta}{dt^2}$, which in their turn lead by double summation to the true values of ξ, η, ζ. By a similar process the values of ξ, η, ζ, for the next argument are ascertained, and so on in succession, the operation being rapidly carried on throughout the entire period for which the perturbations are to be computed.

In order to test the practical utility of this method, M. Encke compared its results with those obtained by the method of the variation of arbitrary constants. For this purpose the perturbations of *Vesta* by *Jupiter* from 1854, May 24, to 1855, October 7, were subjected to a double computation. The following table exhibits the values of the polar co-ordinates of the disturbed planet at the end of this period, as resulting from the use of both methods:—

	Variation of Constants.	New Method.
	° ′ ″	° ′ ″
Longitude	335 20 6·4	335 20 6·6
Latitude	−5 40 42·3	−5 40 42·3
Log. Rad. Vector	0·3665032	0·3665042

M. Encke mentions as a proof of the easy application of this method, that in computing the perturbations of *Vesta* with given places of *Jupiter* for thirteen epochs, only eleven hours were occupied, notwithstanding that he was frequently interrupted in the course of the operation.

If extreme accuracy is required, the values of x, y, z, resulting from the application of ξ, η, ζ, to x°, y°, z°, may be employed in computing X, Y, Z, and then by repeating the operation we may obtain corrected values of ξ, η, ζ. This process, which gives the perturbations depending on the square of the disturbing force, may be repeated over and over again until the assumed and resulting values of ξ, η, ζ, finally agree. The method, however, is so rapidly exhaustive that this object is generally secured at the first repetition.

When the perturbations are large the method is still practicable, but it becomes necessary in this case to reduce in a corresponding degree the magnitude of the intervals of quadrature.

The following are some of the principal advantages attending this method of computation: —

1. It proceeds from the fundamental equations of motion by a series of arithmetical operations of extreme simplicity, only a few analytical expressions of by no means a complicated character being employed in the computation.

2. It affords the means of immediately correcting the quantities employed in computing the disturbing forces for each successive interval, and thereby leads rapidly to results of great accuracy.

3. Since the perturbations of x, y, z, are of the same order as those of the polar co-ordinates, they serve to afford unequivocal indication of the extent to which the intervals of quadrature ought to be diminished. It is well known that the method of the variation of constants may assign enormous perturbations to the elements while at the same time the actual perturbations may be very insignificant.

The original researches of M. Encke on this subject are contained in Nos. 791, 792, and 814 of the *Astronomische Nachrichten*. It may be mentioned that a similar method had been already proposed by Mr. G. P. Bond, of Cambridge, U.S. (*Memoirs of the American Academy of Arts and Sciences*, vol. iv. part i. p. 189). M. Encke, however, did not become acquainted with this circumstance until he had completed his investigation.

Extract of a Letter from Mr. Nasmyth on the Red Prominences seen in Total Eclipses of the Sun.

"In reference to the note at page 210, in the last *Monthly Notice*, touching the proposed mode of enabling us to get a *chance* of a peep of the sun's red flames any clear day about midsummer when he is near the meridian; I believe I may claim to be 'the party' who first broached the notion of the dark chamber and the white screen with the hole in it, so as to let the sun's image pour into a black box, and so get rid of his light on the screen, while the image of the red flames might have a chance to be left behind on the screen. I first turned my attention to this subject on reading the accounts of the last total eclipse, July 28, 1851, regretting that so very limited a time was allowed for making any deliberate observations on that occasion. If my plan could be got to succeed, I doubt not it would tend to supply us with such facts as would enable us to get more insight into the nature of the red flame phenomena; otherwise, I fear, it may require ages to afford the requisite opportunities.

"I communicated my plan with sketches to the Astronomer Royal, and as he appeared to enter warmly into the subject, and proposed to try the experiment, I sent him some brass-work for the mounting of the telescope he proposed to devote to the purpose; but I suppose he has been so occupied with more important affairs, that I have not had the pleasure to hear from him whether he has had any result."

[Here Mr. Nasmyth gives the sketch of an arrangement well adapted to the purpose.]

"If no one having the desired means and leisure will make the attack, I shall hope to do so myself some fine midsummer day.

"I sent a sketch to Mr. Hind and Mr. Lassell. I think it was about August 1851 I did so. Pray pardon me troubling you on this subject; but as the matter has been alluded to in the *Notices*, perhaps you may think fit to name the subject in the next. I also proposed it to Professor Piazzi Smyth, who made some experiments, and saw some remarkable pink or rose-coloured appearances; but Mr. Airy is under the impression that these were due to some instrumental agency.

"I am working away at the moon's surface most steadily and carefully; and hope to leave that subject advanced a step or two ere I am called away. My 20 aperture reflector is a noble tool for this work; and I have now a very respectable figure which, weather permitting, yields me glorious treats in rummaging among the banks and braes of the lunar craters. I have with 450 as much light as the eye can well support for a spell of a couple of hours' observation and sketching; so, were the instrument of greater aperture, it would be of no service, in proportion, for this my *work:* for I have determined to devote myself to it, and not leave off till I have left a few marks of progress."

Extract of a Letter from Professor Secchi to Mr. Grant, F.R.A.S.

"*Rome, May* 8, 1852.

"Allow me, dear Sir, to present you my most sincere thanks for the kind letter you addressed to me, and for the valuable present of your book, which was joined with it. I am very far from performing things worthy of the historical renown of our country; but I will try to make the best use of those means which are put at my disposition, although they are, indeed, very scanty: if in other times they were at the level of science, they are now decidedly below it. I hope, however, this state will not last long, and that some substantial improvement will be made to our observatory. The history which you have written is that of the most attractive part of the science, and which was wanted not only in England, but also in Italy, and I have no doubt it will be well received here. I would have written to you sooner, but I waited a little while, to be able to inform you of some researches which I have undertaken on the distribution of heat at the surface of the sun, and on the constitution of the lunar geology, which I hope will be well received by you.

"As to the first subject, you know that it is generally admitted that light is brighter at the centre of the solar disc than at the edges. M. Fizeau proved that the same diminution subsisted for chemical radiation, and I obtained a confirmation of it in the daguerreotypes of the sun taken during the last solar eclipse. But I had no proof that the fact would be the same with heat, although this was highly probable. I tried, therefore, to fill this blank, and have instituted a series of researches on the temperature of the different parts of the solar disc. I have been thus led to some interesting conclusions quite new to me; but before exposing them, I think it necessary to say something on the method of conducting the experiments. A thermo-electric pile, belonging to a very nice Melloni's apparatus, was attached to the telescope of our equatoreal, and fixed on the prolongation of its optical axis at a distance of about 15^{cm} from the eye-piece: the image of the sun, magnified by the eye-piece itself, was thus projected on the pile, and had a diameter of 12^{cm} nearly. When a leaf of white paper was put in the place of the pile, the image was perfectly defined, and the spots neatly terminated. By a diaphragm, the aperture of which was 4^{mm} broad, and 15^{mm} long, disposed parallel to the edge of the disc, a small portion of the sun's image, which was about $1'$ of arc broad, and $4'$ long, was allowed to radiate on the pile. The construction of the equatoreal enabled me to keep the same points of the disc steadily fixed on the pile, as far as it was necessary to obtain an invariable deflection of the needle of the galvanometer, and the position of the points of the sun, under examination, was readily deduced from the reading of the declination circle. There is no need to observe that all extraneous radiation was pre-

vented by large screens, supported by the telescope itself, and the turning dome was only opened as far as it was necessary to introduce solar rays on the object-glass. The telescope has $1^{m}\cdot25$ focal length, and 77^{mm} aperture.

"Now these are the results obtained in sundry experiments: 1. The heat of the solar image is at the centre almost twice as great as at the borders. This is found to be true, examining the diameters both in right ascension and declination; 2. The maximum of temperature did not appear to be at the centre, but above it, in a point distant from it about 3′ of geocentric declination. Constructing graphically the curve of the intensity of heat, taking as *abscissæ* the parts of the sun's diameter, and as *ordinatæ* the intensities themselves, it appears that this curve (a kind of inverted parabola) is not symmetrically disposed about the axis of the ordinates, but a good deal inclined towards the upper edge. I subjoin some numbers which represent the intensity of heat in the parts of the diameter of the sun taken in minutes, + above and — below the centre of the image.

Positions on the diameter of the sun in declination	+14′·96	+11′·32	+ 3′·00	+ 1′·32	−10′·9	−14′·88
Relative intensity of heat	57·39	88·81	100·00	99·48	81·32	54·34

These are the results of eight series of experiments, none of which is found in contradiction with the others, and their separate numbers are very nearly the same, so that the fact seems to me completely ascertained. It is certainly curious that the maximum of heat corresponds with the position of the solar equator, as visible from the earth at the epoch of the experiment (20th, 21st, 22d March). This leads naturally to the conclusion that the solar equatoreal regions must be hotter than the polar regions, as was suspected already from the more frequent appearance of the spots there. The conclusion seems perfectly accurate, even admitting a solar atmosphere, since the effect of this last should be to diminish symmetrically the radiation around the centre of the image; on the contrary, if the polar regions are less hot than the equatoreal, the intensity of heat should have been less in the lower part of the image, where the south pole of the sun was visible; and, consequently, the parts having equal distance from the centre of the image had a very different heliographical latitude, on account of the inclination of the solar axis to the ecliptic. From these principles only, the non-symmetry of the curve is accounted for. If *this alone* is the cause, the curve will be found symmetrical in the months of June and December, and reversed in September, since in the two former the equator passes through the centre of the image, and in the last is below it. But it is not impossible that the two solar hemispheres should possess different temperatures, as seems to be the case on the earth, and is suspected in *Mars*. If this is the case, these researches will throw some light on the climatology of the earth itself; since the heat of the sun must be different, according as one or the other of its poles is turned towards the

earth. Future experiments will resolve this question. With respect to the poles of the sun, I shall add here a conjecture on a fact recently discovered by Captain Sabine. The journal *Institut* relates that this gentleman has found that the deviation of the magnet from its mean position at the Cape of Good Hope is found to be in opposite directions at the epochs of the two equinoxes. Might this not be an effect of the solar magnetical polarity on the terrestrial magnetism? The fact deserves to be examined, if it takes place in our hemisphere, and in opposite directions. Coming again to the solar heat, I have found that spots seemed less hot than the rest; but as only small groups of them were visible, no singular fact or law can be stated from these observations. I shall conclude this account by noticing an odd historical coincidence, namely, that these observations were made in the same room where it is said F. Scheiner, the first who used a telescope mounted equatorially, made his observations of the sun. This room has been this year added to the observatory.

"I shall now briefly expose to you some considerations on the theory of lunar formations, which have been the subject of some discussions with our eminent geologist, Professor Ponzi, whom I gladly engaged in these researches. He is exceedingly well acquainted with the terrestrial volcanoes, both ancient and modern, (being now occupied in the description of the Roman States), and with Italian geology: the conclusions to which we have arrived are these.—The lunar volcanic formations must be distributed into three classes, to which a fourth class may be added analogous to our Plutonian formations.

"The first class of the lunar volcanoes possesses a distinctive character; that the edges of the craters are almost completely obliterated, so that their border now is a continuation of the plane ground, in which they seem excavated, and a deep well only remains in the place of the ancient mouth of the volcano. Instances of this kind are very frequent near the south pole of the moon, and around the large spot Tycho; but Tycho itself does not belong to this class. The physiognomy of these craters nearly resembles our submarine volcanoes of the Monti Ciminii to the N.W. of Rome. The country around the craters of Bracciano, Bolsena, di Vico, is almost flat, and the old openings of the craters are now deep lakes. On this ground we are led to believe that even in the moon many subaqueous volcanoes existed. Another distinct character of these volcanoes of the first class is, that they are in a line, as if they burst from the cracks of the solid body of the crust produced by earlier formations: this is most striking in Arzahel, Purbach, Alphonsus, and many others, and they seem to follow the cracks made by the *soulèvement* which raised Tycho, the lunar Appennines, &c. Some of the higher chains of lunar mountains are seen visibly parallel to the alignement of the craters: this fact also is like that which we observe on the earth; indeed, the large Italian volcanic chain follows the line of the Apennines along this country.

"The second class of lunar volcanoes are those which have their outside edges elevated above the surrounding plane; their form is generally regular, and not broken, as those of the preceding class, and the ground around them is elevated in a radiating disposition, as is visible around Tycho, Copernicus, Aristotle, &c. The regularity of their forms suggests that the ejected matter was not disturbed by the motion of waves, and, consequently, that they were atmospherical volcanoes, like those of the Monti Laziali, Albani, and Tusculani, at the south-east of Rome; the want of breach in the craters seems to indicate that no lava, but only scoriæ and loose matters have been ejected. The disposition of the soil around them suggests the opinion that they are of a comparatively later epoch, and formed after the crust of the satellite was pretty resistent, and was capable of being elevated all round by a great effort. It is singular, indeed, that this *radiation* of the soil around is found proportional to the magnitude of the central crater. The effect of this *soulèvement* extended sometimes to a prodigious distance, comparable to that of the Cordilleras of the Andes on the earth. The greater part of the craters of both the classes now described possesses an insulated rock inside, very seldom appearing (at least in commonly good telescopes) perforated. This bears great analogy with what we see in more than one place in the ancient volcanoes of the earth, where the erupting mouth has been stopped by a dome of trachytic matter as by a stump. Monte Venere, near Rome, is of this formation, and lies in the centre of an immense old crater.

"The third class of lunar craters is very small, and bears a great likeness with those called by geologists adventitious craters, and seems to be of a very late formation, the last efforts of the expiring volcanic force. They are irregularly scattered through all the moon, but occur more frequently at the borders or inside of the old demolished craters, although not concentric with them, and seem to have been produced after the large ones were completely closed, either by trachytic ejection or by becoming lakes. These small craters have very seldom rocks inside, or a flat bottom; but their cavity is conical, and does not exceed in dimension our common volcanoes, which are yet active on the earth. From these facts and observations it appears, that volcanic action has gone on in the moon through all the same stages which it has gone and is going on in the earth, and is there, probably, completely extinguished, on account of the smaller mass of the moon, which has been cooled very rapidly. This rapidity of cooling, joined with the smaller gravity, may account for the great developement of volcanism there, and comparatively fewer Plutonian formations. But extensive instances of this kind are not wanting; the lunar Alps, the Apennines, the Ripheæ, &c. may represent this formation, surrounding vast basins, and having modern volcanoes following the direction of the higher edges of their chains. Professor Ponzi seems to think it unquestionable that water existed at the surface of the moon; the fierce glare of the sunshine is not able to melt the ice there, which is, probably, at the temperature of the planetary spaces; just

as the sun at the surface of the earth is not able to melt our glaciers, which yet possess a certainly higher temperature. Cold, and other unknown causes, may have absorbed and fixed all the atmosphere which anciently existed, as we see that the immense atmosphere which anciently surrounded the earth has been fixed by several chemical processes and reduced to its actual composition; and it might be possible that this actually existing atmosphere of ours should be all solidified, either by cold or chemical processes, if the earth arrives at the same degree of cold which seems to have place on the moon.

"These are some of the conclusions to which we have arrived in our inquiry into the solar and lunar constitution; they do not certainly belong to astronomy strictly taken, but, a recreation from more serious occupations may be excused to astronomers dwelling in the pure climate of Italy. I trust you will receive them as a token of my gratitude, and of interest for the work which you have published."

In a later letter Professor Secchi tells Captain Manners "that the observatory of the Collegio Romano is about to be furnished with a large refractor equatoreally mounted. The refractor will have 9 inches aperture, and a focal length of 14 French feet." There is only a little hesitation at present as to the best mode of mounting this telescope.

Observations Héliothermiques, par Sig. Secchi.

(*Comptes Rendus, Oct.* 26, 1852.)

Since the date of the preceding communication, Sig. Secchi has made the following interesting experiments on the heat emitted by different parts of the solar disc. It appeared from the former experiments that the heat increased from the margin of the solar disc to the centre; but there was this peculiarity, that when it was measured in the direction of the axis of the sun's rotation, it attained its maximum intensity at a point situate about 3′ above the centre. At the time when the former experiments were made, the projection of the sun's equator upon his disc passed above his centre, at a distance agreeing almost exactly with the position of maximum temperature; whence the conclusion naturally suggested itself to Sig. Secchi, that the equatoreal regions of the sun are intrinsically hotter than the other parts of his surface. He justly remarked, that if this were true, it ought to follow that in the month of June, when the nodes of the solar equator pass through the earth, the upper and lower hemispheres should appear equally heated; and on the other hand, in the month of September, when the projection of the solar equator attained its greatest depression, the point of maximum temperature should fall *below* the centre of the disc. Experiments similar to those previously undertaken by him have served to confirm this view of the subject. "In the month of August the upper hemisphere appeared a little hotter than the

lower, but the differences, always very small, were not unfrequently negative. In September *the lower hemisphere appeared decidedly the hotter of the two.* The maximum difference rather exceeded 2° (by the galvanometer), being almost identical with the result obtained in the month of March *in the opposite direction.* Moreover, this maximum occurred from the 14th to the 16th September, which also corresponds with the epoch of the greatest depression of the solar equator."

Sig. Secchi remarks, that the influence of the solar spots upon the temperature is very striking. Sometimes a spot, which did not occupy more than $\frac{1}{100}$th of the aperture of the pile, caused the temperature to fall 3°, or about $\frac{1}{15}$th of the whole intensity. He found, however, by isolating the spot with a diaphragm, that it exercises a sensible influence even in the obscure part.

Extract of a Letter from Mr. Lassell.

"Valetta, 1st Nov. 1852.

"You will, I hope, have received a letter I wrote to you shortly after my arrival, giving you my first naked-eye impressions of this climate. I am now enabled to say that I find it quite as favourable for the use of the telescope as I had anticipated—incomparably more so than at Starfield, both in respect of quantity and quality of clear sky. My first observation through the telescope was on the 5th of last month, but it was not completed in respect of clock-motion and driving apparatus until the 16th, and on the 26th we were visited with a tremendous "*gregale*," which, beside causing some shipwrecks with loss of life in the harbour, and upsetting a whole battery of 24-pounders as if they had been ninepins, gave my roof a tremendous shake, and, partly dislodging it from the wall, would, I believe, have carried it quite away, if it had not "brought up" against the strong frame-work of the polar axis. Happily no injury was done to the telescope beyond crushing the tube of the finder, and that without injury to the optical part. This has lost me some eight or ten days; but all the mischief is now repaired, and since the 27th I have observed every night, the weather having settled again so as to be even finer than before the storm. The principal objects viewed are, as you may surmise, *Neptune*, *Uranus*, and *Saturn*, the last being an object of such surpassing interest as to absorb almost more than its share of attention. I send you herewith a careful drawing of its present appearance; and, if viewed through a blackened tube with one eye, at the distance of three or four feet, it presents so exact a fac-simile of the picture afforded by the 20-foot telescope and a power of 650 that I know not how to introduce a correction. I have compared it carefully with the telescopic image, by having it well illuminated and placed alongside the eye-piece, in order to form this estimation, and the only thing that suggests itself as a possible improvement of its accuracy, would be *a very slight* increase of breadth of the outer

ring at the expense of the breadth of inner bright ring. To rival the original, however, in *beauty*, I find to be impossible, especially from want of proper colouring. The two principal equatoreal belts are of a ruddy brown colour, changing pretty suddenly at the southern edge of the most southern of these belts into a dusky bluish-green colour, very much deepened around the south pole, the exact place of which seems to be marked out by a circular lighter shade. This dusky portion is much variegated by minute dark stripes and lines, which defy imitation with the pencil. One of the most striking attendant phenomena which I now note for the first time, is the *evident transparency of the obscure ring.* [I intend in future, in speaking of the rings, to adopt M. Otto Struve's nomenclature, calling the outer ring *A*, the intermediate *B*, and the obscure ring *C*.] The ring C, crossing the ball, is of a much lighter texture or colour than the other parts, and both limbs of the planet can readily be traced through it, and quite as plainly as in the drawing, when the latter is treated as I have pointed out. The effect is precisely that of a band of *crape* stretched within the ring, which, projected on the white ball, would appear of a lighter hue than where projected on the dark sky. There is evidently, also, a sudden paleness of the main division of the rings where it crosses the ball—scarcely sufficiently marked in the drawing—as if in some degree, at least, the ball were seen through the division. The shading on the inner edge of the ring *B* extends clearly within one-third of its entire breadth of the outer edge, and during some rare glimpses appeared to me as if it were constructed in narrow concentric circles. No division, properly so called, could at any time be traced or imagined on the ring *A*; but there was an evident *shade* in the middle of its breadth, and occupying about one-third of it. This was seen night after night with different powers, from 297 to 650, so that I am perfectly satisfied of its existence, nearly as shown in the drawing. Its depth of shade is rather deeper than the darkest shading of *B* close to its inner edge.

"A remarkable sudden projection of the shadow of the ball on the ring, where it crosses the division, was noticed especially from the 5th to the middle of October, when the shadow was much larger than it now is. It is just indicated in the drawing, but not so palpably as in the original when best seen. I am not able to say whether or not it is an optical deception, arising from the juxtaposition of the division and shadow. A more remarkable phenomenon was repeatedly noticed on the opposite limb, appearing like an elongated spot, sometimes extending up to the division, but generally falling short of it, and projected on the ring *B*. That something was there I am persuaded, but I can offer no explanation of it.

"I must now allude to a most extraordinary anomaly, which I confess myself utterly at a loss to explain. Notwithstanding the exquisite views I have had of the planet since my arrival here, far exceeding in sharpness of definition all I have ever seen at Starfield, I have never obtained a single glimpse of the division in the ring *C*,

seen and measured micrometrically by M. Otto Struve; nor of the ring being concentrally divided into two, as seen by Mr. Dawes. On every occasion, and when most sharply defined, it appears of one uniform texture and depth of shape, constantly conveying to my mind the idea of network or a crape veil, as I originally described it. Moreover, when best seen, the part crossing the ball seemed of uniform density, the northern edge not appearing sensibly darker than the southern.

"If what I have described should not give an adequate notion of the amount of precision or sharpness of the telescopic image, I may add, that *Enceladus*, even when over the ball, and now not more than the narrowest breadth of the ring from it, is not to be overlooked; and *Mimas*, when at a considerable distance from his greatest elongation, is steadily seen with various powers, from 297 to 650, with the splendour of the planet fully view. The vast difference of visibility of *Mimas* and *Enceladus* is only to be appreciated by those who have seen both together.

"The superiority of vision I am now enjoying is not confined, however, to *Saturn*; I am enabled to watch the satellite of *Neptune* every night—completely round his orbit. On the 30th of October I observed him very near his closest approach to the planet, and certainly then with extreme difficulty, being, as nearly as I could judge, not more than eighth-tenths of the planet's diameter from his limb. My usual micrometer powers, of 614 and 760, failed to show the satellite, and it was only after trying a succession of single lenses that a power of 1917 at last brought it out. The two new satellites of *Uranus*, too, *Ariel* and *Umbriel*, I have been enabled, not only to see, but to measure micrometrically—a thing I was never able to do in England, as I was there always obliged to hide the planet in order to see them. I hope, therefore, to get a large and good series of measures of these and of the satellite of *Neptune*, as well as of the old satellites of *Uranus*, the results of which I will send you in due time.

"I believe, however, that *all* the advantages of vision of which I speak do not arise from the climate and greater altitude of the objects, but in some sensible degree from the increased perfection of the figure of the speculum. You will remember I some time ago mentioned that I was engaged in some attempts to improve the action of the polishing-machine, in producing the utmost attainable *regularity* of curve. After a great many elaborate contrivances, none of which satisfied me, I hit upon a very simple modification, which, I think, answers the end perfectly, and enables me to produce far more perfect figures, and with much more ease and certainty than formerly. I intend to take an early opportunity of sending you a description of the mechanism superadded to the machine. After the addition of this improvement, I refigured, shortly before I left home, specula *A* and *C*, both of which I brought with me, but as yet I have only worked with *A*.

"I presume that, unless another hurricane blows away my observatory, I am located here for the next three or four months."

Extract of a Letter from Mr. Lassell to Mr. W. De la Rue.

"*Valetta, 8th Nov.* 1852.

"My dear Sir,— I am ashamed of having so long delayed to thank you for the 'skeleton forms' of *Saturn* you have been so kind as to send me. How did you draw the outline so perfectly? It seems to me the best representation of a circle in perspective I have ever seen.

"I sent a drawing of the planet to the Astronomical Society, which may possibly be on the table at the meeting of this month, exhibiting such phenomena as I had at that time been able to make out, using your 'form' as a scale, and making such changes in the proportions of the ball and rings as I thought necessary. Your drawing, however, I think generally so correct, that the variations I have introduced are but small. As I sent a pretty long letter to Mr. Sheepshanks, detailing what I had then seen, it is unnecessary to repeat what you will already have heard; I may add that I was then able to state the result of only a very few observations, and those made under the disadvantages of an unfinished observatory. I am happy to tell you, that both in respect of the skies here and of the performance of the telescope, I have reason for the highest satisfaction. Since the restoration of the Observatory, on the 27th ult., after the storm, there has not been a single cloudy night, nor one unfit even for delicate work; so that you may suppose that observing, recording, and taking needful repose, leave me scarce a moment's leisure. The nights are as remarkable for tranquillity as transparency; and I have not encountered one of those nights so frequent at Starfield, on which I only opened the Observatory to close it in disgust.

"The satellite of *Neptune* is a bright object, not to be for a moment overlooked by the most cursory observer, and the two new satellites of *Uranus* are constantly conspicuous in the field of an eye-piece magnifying 1018 times, which is, indeed, the best power, in the finer states of the air, for exhibiting both planet and satellites. I am enabled also to add that the figure of the speculum appears to be all that could be wished or desired. It is the first fruits of the transverse motion superadded to the polishing machine, rendering it in my judgment a perfect engine for figuring specula. The night of the 4th inst. when the air was somewhat better than usual, *Saturn* was exhibited in a style that beggars description; power 650. The number of minute belts and stripes all over the ball defies my delineating powers; but I shall try to record the main phenomena.

P.S. "The night of the 4th *confirmed* all I had previously seen, but scarcely *added* anything more, except revealing more minute and numerous markings on the ball."

Letter from Mr. Lassell to Mr. Sheepshanks.

"*Valetta, Nov.* 25, 1852.

"Since my letter to you of the 2d, I have been favoured with one or two nights of finer quality than usual, especially the 4th inst., when the planet *Uranus* and his four satellites were exhibited with a power of 1018 in a style I never saw before or ever hoped to see. The near satellites were bright objects, and not for a moment to be overlooked. The disc of the planet was harder and sharper than I have ever seen it with any power in England, yet I could not satisfactorily make out any phenomena upon its surface. Under these favourable circumstances I carefully surveyed the planet round to the distance of $2'\frac{1}{2}$, without finding any points of light which might probably be other satellites. I obtained on this occasion good micrometrical measures in position and distance of all the four satellites, and also some measures of the diameter of the disc.

"The view of *Saturn* with 650 left nothing to desire in hardness and sharpness of definition, except the removal of some slight atmospheric disturbances rendered visible with this power. So far as the telescope was concerned, no defect whatever of the image was appreciable. The view with 22 inches was compared with that afforded by the full aperture, and the latter was preferred, the outside annulus appearing to add to the light and sharpness of the image, though it diminished the blackness of the field. A number of minute and exquisite *markings* on the southern portion of the ball, together with three distinct dark blue or bluish-green belts, were for the first time distinctly noted; and both limbs of the planet were most distinctly traced *through the obscure ring*, quite up to the inner edge of the inner bright ring. What can this marvellous and unique appendage—the obscure ring—be? I do not venture much into the field of speculation myself, but I can scarcely help wondering whether this sort of tangible nebula can be a connecting link between the more attenuated nebulæ of the universe, and the solid bodies of our planetary system?

"In other respects, the views of *Saturn* obtained on this remarkably fine night did not add much to the phenomena I had previously seen by glimpses; but my attention became more directed to the delineation of the belts and phenomena of the ball; and between this night and the 13th inst., I made a number of observations, which by means of description, and a sufficiently rude drawing sent herewith, I will attempt to put upon record. First, then, referring to Mr. De la Rue's diagram, and reckoning southward from the southern portion of the obscure ring where it crosses the ball, there is the brilliant zone about equal in breadth to the sum of the two bright rings at their narrowest part. Secondly, the principal ruddy belt extending from about 7° to 28° south latitude. Thirdly, a white zone about 5° broad, not, however, so brilliant as the first zone, nor so uniform and well defined in its brightness.

Fourthly, a yellowish ruddy belt terminating sharply about 45°, in. Fifthly, an extremely narrow dark blue or bluish-green belt, Sixthly, another light zone, but of a deeper shade, and marking the commencement of the bluish-green portion. Seventhly, a lighter narrow blue belt passing through the 60th degree. Eighthly, a still deeper-shaded light zone. Ninthly, a sharply-defined, very dark blue belt, a little above the 70th degree, and encircling the pole. Of this belt about three-fourths of the whole circle can be seen, including within it the pole itself, marked by a lighter-coloured spot of about 20° diameter. Thus, if the less shaded parts, or those I have called zones, be the body of the planet, there will be distinctly traced *five belts*,—two ruddy and three of a neutral tint or bluish-green colour. I scarcely know how to apologise sufficiently for the rudeness of the drawing; but not being an artist, I have not attempted to produce an artistic effect, but only to convey a correct idea. And I have, I believe, only further to observe, that while I have not attempted to introduce the more delicate lines, something like what I have drawn undoubtedly exists on the planet, though softened down and made infinitely less *staring* than it is in the drawing.

"The ambiguous 'spot' on the south-following portion of ring *B*, close to the ball mentioned in my former letter, appears to have been the germ of the coming shadow, which, on the 15th inst. had become plain on the following portion of ring *B*, without being visible on ring *A*, and while the shadow still lingered on the preceding side. This lingering shadow had now assumed the form which the 'spot' previously spoken of presented in the early part of October, and would appear to be the 'remarkable sudden projection of the shadow of the ball' there spoken of. Whether or no this projection indicates, as has been suggested, some peculiarity of form of the edges of the rings, I certainly saw the shadow itself fairly entered on ring *B* on the following side, while it still remained visible as a fine line on the preceding side.

"I shall obtain, I doubt not, some additional testimony about the orbit of *Hyperion*, having watched him through an entire period, and obtained measures of him even at both his opposition and conjunction.

"This is a marvellous climate, a single month yielding almost three times as many observations of the satellites of *Uranus* and *Neptune* as I got all last year! The last three days, however, the weather has broken, and we have violent gusts of wind and tremendous showers of rain."

On the Appearances of Saturn. By Mr. Dawes.
With a Drawing.

"The accompanying sketch of the planet *Saturn* represents the principal phenomena seen by me during the month immediately subsequent to the 19th of last September. One of Mr. Warren

De la Rue's lithographed outlines (with some of which he kindly supplied me) has been employed, as facilitating comparison with sketches by other observers, by whom the same outline may be used. I have, however, in this sketch considerably reduced the breadth of the projection of the obscure ring on the ball, as it was originally much broader than it appeared in my telescope.

"The autumn has hitherto presented few favourable opportunities for the use of high telescopic magnifiers; but the nights of September 20th and 25th were very fine, especially the latter. Some of the notes entered on those nights I will here transcribe from my journal.

"'1852, Sept. 20. $8\frac{1}{2}$-foot achromatic. The faint ring at times comes out well. There appears to me to be a *dark line* between it and the bright ring. Power 366.

"'The inner edge of the inner bright ring has, as I observed last year, a very narrow line, considerably brighter than the portion immediately exterior to it: in other words, the dark shading of the inner ring towards its interior edge does not extend quite to the edge; or, at least, it is not so dark at the edge' (illustrated by a diagram in journal). 'The dark shading has a very defined appearance, like a belt with rather sharp edges.

"'The two belts on the ball just south of the equator are pretty well defined; but the most southern is not much darker than the cap-like belt which covers the south pole.'

"'Sept. 25. $8\frac{1}{2}$-foot achromatic. *Saturn;* power 460; single convex lens. Extremely distinct when the fog is not too thick to afford sufficient light. A very narrow dark line on the outer bright ring; the portion 'of the ring' exterior to this line is *less luminous* than the portion interior to it. The brightest part of the outer ring is that close to the division between it and the inner bright ring.

"'The shading on the inner bright ring is *streaky;* and it extends towards the exterior edge of the ring to within about one-fifth of its whole breadth. A dark and distinct shading, like a sharply-defined belt, is seen *near* the inner edge, but the edge itself is obviously brighter, though the brighter portion is extremely narrow.

"'The dark ring is visible, but is very faint, owing to the fog; and occasionally its exterior portion only is seen.

"'The projection of the dark ring on the ball is by no means *black;* neither is the southern edge of the dark line much, if at all, blacker than the rest of it. It appears as though the *shadow* of the faint ring were not really *black*, rendering it probable that the faint ring may be *partially transparent*. There is a striking contrast between the dark gray projection of this ring and the shadow of the ball on the bright ring, which is perfectly black.'

"This last paragraph gives the result of my endeavours to discern a difference of colour between the *projection* of the faint ring on the ball and the *shadow* which it must at that time have cast *beyond its own inner edge;* and which I expected might possibly be perceived as a *black* line distinguishable from the *gray* projection. But though the whole breadth of the dark line, made up of the

faint ring and its shadow, was obviously broader than could belong to the projection alone, no difference could be perceived in the degree of darkness.

"On three fine nights in October, being the 18th, 19th, and 20th, I noticed a well-defined narrow belt at about 40° or 45° of south latitude on the planet. This I did not observe last year. The division of the broad belt (just south of the equator) throughout its whole length by a narrow bright line is more distinct and uniform than it usually was last year. Towards the close of last year's apparition I noticed that it had become more distinct in some portions of the planet's circumference than in others.

"The belt-like cap which covers the whole of the southern hemisphere is considerably less dark near the south pole, agreeing in this respect with the appearance last year. But when best seen, the greater part of the hemisphere appears streaky in the direction parallel to the equator.

"On the above-mentioned fine nights in October, my friend the Rev. C. Strong, who was observing with me, frequently had his attention arrested, while looking at other objects (the faint satellites especially), by the dark line on the outer ring; and remarked that it gave the impression of a division of the ring at a point a little exterior to the middle of its breadth. It is certainly remarkable that this line has continued to be visible in my telescope under fine circumstances for the last two years; while the far larger telescopes of the Harvard and Pulkova observatories, and the Starfield 20-foot reflector, have all shown the ring without even a variation of shade throughout its breadth. Is it not possible that in telescopic, as well as in microscopic observations, a certain quantity of light may be the most favourable for the discerning of extremely narrow dark lines on a luminous ground? Certainly, in my own telescope, such delicate phenomena are not most plainly seen when the planet is most brilliant; but rather, as on the exquisite night of Oct. 26, 1851, when the heavenly bodies are shorn of much of their usual splendour.

"*Wateringbury, November 1852.*"

"Since the date of the observations which I communicated to the Society in the beginning of November, few opportunities have occurred of making further acquaintance with the phenomena of *Saturn*. On Nov. 16th, the planet was seen bright and clear between clouds for a very short time, during which the semi-transparency of the obscure ring was distinctly recognised with my refractor, power 323. I had that morning received from Mr. Lassell (under date 'Malta,' Nov. 9), a long and interesting account of his discovery of this fact; but expected that it would probably require the finest possible circumstances to render it distinctly visible in my telescope. I was, therefore, agreeably surprised to find it by no means very difficult to make out in a

moderately good state of the air; and on the 18th, and especially on the 30th, it was seen satisfactorily. The edges of the ball were distinctly traceable down to the inner edge of the bright ring; the projection of the obscure ring having much the appearance of a very dark and distinctly defined belt. Such it was, doubtless, formerly supposed to be; and it is so delineated in Professor Encke's pictures of *Saturn* in the *Transactions of the Berlin Academy for* 1838.

"The dark shading which has for some time surrounded the southern pole appears to me to have formed itself into a more distinct belt than was visible in September. I believe the whole, or very nearly the whole, of this belt is seen, the middle of its breadth being about 12° from the pole, which is comparatively light. On November 30th, the whole region within 30° or 40° of the pole was noticed to have a light greenish blue tint, the tendency to green being more decided than I had before observed. The night was at times very brilliant, and *Enceladus* was distinctly seen.

"The observed semitransparency of the obscure ring has greatly strengthened an idea I have long entertained, founded on its situation and small reflective power, that it may probably be composed of water or some similar fluid; and if this notion be correct, we may reasonably imagine that its condition may be very variable; and, especially, that divisions seen in it at one time may not be recognisable at another. In estimating the degree of its transparency, it must not be overlooked that the light on the portion of the ball, which we see through it, must have passed through its substance *previously* to its reaching the planet, and thus has been *twice* transmitted through it before it comes to us.

"*Wateringbury, December* 1852."

Letter from Rev. W. R. Dawes to Mr. De La Rue.

"Saturday evening was brilliant here, but by no means sufficiently favourable for delicate observations or measurements. I attempted the latter, but with no satisfactory result. The edges of the planet were soft with the requisite power, and their flickering at the webs of the micrometer defied my attempts to get a certain observation. I then tried the double-image micrometer with little better success: two observations of the polar diameter gave 19″·13, and two of the equatoreal gave 20″·56; both too large, I have no doubt, as might be expected from the diffused state of the limbs. These obs. give

$$e = 0{\cdot}06955 = \frac{1}{14{\cdot}38}.$$

"I shall try again under better circumstances, when such are to be had.

"Thanks for the communication of your idea respecting the faint ring. I could enter upon no such niceties on Saturday. But,

if you will refer to observations I long since made and published, you will find as strong a confirmation of your happy suggestion as you can desire. or are likely to receive from future observations, except under circumstances of rare occurrence. In the *Monthly Notices* for Jan. 1851, page 52, you will find I have said, 'I have always observed that the upper (southern) and more distant portion of the obscure ring is more plainly seen than the corresponding portion on the side nearest to the earth, and also that the projection of it at its minor (misprinted *inner*) axis is considerably narrower than accords with its breadth at the major axis.' In writing at the same time to the *Astron. Nachrich.* (see No. 751), after stating as above, I have added, 'To avoid the irradiation in diminishing its (the projection's) apparent breadth, I have carefully examined this dark line in daylight, and strong twilight, when the air was quiet and the image sharply defined. By comparison of its breadth with that of a spider's-web of the micrometer, I concluded it to be not more than from 0″·2 to 0″·25 at the minor axis, which, for the time of observation, corresponds to a breadth of from 1″·08 1″·35 at the major axis, instead of nearly 2″ as I had measured it. The appearances would be satisfied by supposing that a section of the obscure ring may be somewhat *wedge-shaped*, the thicker edge being outwards, and that a section of the bright ring may also be wedge-shaped, its thickest edge being inwards.

"But all this bears directly on your supposition that the obscure ring is not in the same plane as the others; and the observations are better satisfied by it than by any other hypothesis.* That the projection on the ball is not seen as broad as it ought to be I have always observed, and mentioned it in my last paper with reference to your diagrams of *Saturn*, in which the projection required to be *narrowed* to agree with the view my telescope gives me; and in the beginning of 1851 the difference appeared much greater than it does now, which would agree with your suggestion. I did not, indeed, notice that the *southern* part of the faint ring *encroached* on the *inner edge* of the *bright ring*. But in every other respect nothing could more fully support your hypothesis than the observations which I have uniformly made. Some other things strike me about it, which I have no time to mention now, being obliged to conclude hastily.

"Yours truly,
"W. R. Dawes."

"*Wateringbury, Nov.* 29, 1852."

Second Letter from Rev. W. R. Dawes to Mr. De La Rue.

"While I was writing to you yesterday, it occurred to me that in 1848, while the ring of *Saturn* was invisible, I had observed *two*

* Mr. De la Rue states, that this hypothesis is founded on his observations of the *encroachment* of the dark ring on the southern portion of the inner bright ring and its apparent *contraction* on the opposite side; also on the circumstance of the apparent ellipticity of the dark ring not being the same as that of the bright rings.

dark lines crossing the equatoreal region, one darker than the other, and separated by an exceedingly fine bright line. I had at that time explained the appearance by the supposition that the *lighter* of these two lines was the *projection of the rings* on the planet, and the *darker* their shadow. (See *Monthly Notices*, vol. x. p. 48, under dates Oct. 6, 26, 30; and Nov. 21; and Remark 3, at the end of my paper). It struck me as possible that your suggestion might afford a different explanation of the appearance, and thereby gain additional support to itself. On referring to my observations at that epoch, and considering the relative positions of the sun, the earth, and the planet, and also how the faint ring must have lain at the time with reference to the other rings on your hypothesis, I find that the explanation I gave at the time must have been the true one, and that the faint ring must have been *enveloped in the shadow* of the others, if its plane deviates as you suppose.

"I recollected also a figure of *Saturn* at the same epoch, by Julius Schmidt, of the Observatory of Bonn, in which are shown two similar dark lines to those I described as observed by myself. (See *Astron. Nachrich.*, No 650, and illustrative lithograph, fig. 17). But the same explanation applies to that which applies to my own; so that this support to your hypothesis fails. Nothing, however, arises from this *in opposition to it*; as, even supposing your idea to be correct, the faint ring must then have been simply invisible.

"From Mr. Johnson's measurements of the equatoreal and polar diameters I deduce

$$e = 0{\cdot}09940 = \frac{1}{10{\cdot}06}$$

as an approximate result, omitting the reduction of the polar diameter measured to the true semi-minor axis.

"In like manner, from your measures, we have

$$e = 0{\cdot}08139 = \frac{1}{12{\cdot}29}$$

"Both of these results are considerably less than that deduced by Mr. Main in 1848, viz.

$$e = 0{\cdot}10838 = \frac{1}{9{\cdot}227}$$

"I suspect that, without great caution, the polar diameter may be *squeezed too close* in the double-image micrometer and in the heliometer. The *brightness* of the poles is so much less than that of the equator, that, without special precautions, the measurements of the polar diameter will almost certainly be taken too small. Those with the P. W. micrometer are not quite free from the same danger, for the inflexion of light at the webs is much more obvious at the bright edges of the equator than at the dull polar limbs, and the *irradiation*, too, will be greater for the same reason. I, therefore, *greatly doubt* whether the ellipticity be anything like what Mr. Main deduced. Draw out such an ellipse, and I think you

will say you never saw *Saturn* of that figure. And the eye may sometimes be trusted rather than the micrometer. Recollect, too, that the ellipticity of *Jupiter* deduced from the Greenwich observations from 1840–1848, is little more than *half* what Mr. Mr. Main's observations assign to *Saturn*, viz. $\frac{1}{17{\cdot}9}$.

"*Wateringbury, Nov.* 30, 1852."

Sur les Dimensions de Saturne, par M. Otto Struve.

(*Extrait des Mém. Acad. Scien. St. Pétersbourg*, tom. v.)

In this memoir M. Struve has given the results of a limited series of micrometrical measures of *Saturn* and his rings, which he executed at Pulkowa with the great refractor in the autumn of the year 1851. He also examines some observations of preceding astronomers, tending to throw light upon the physical constitution of the rings.

The obscure ring was very distinctly visible on these occasions. M. Struve perceived that at the extremities of the *ansæ* it was divided into two parts by a dark line. He is of opinion that the division extends throughout the entire circuit of the ring. The gradation of light from the ancient bright rings to the obscure ring was very abrupt and striking; but he was unable to discover the slightest trace of a division between the exterior edge of the obscure ring and the interior edge of the inner bright ring. The breadth which he assigns to the obscure ring (2″·035), agrees almost exactly with that which Dr. Galle, of Berlin, obtained in 1838, and with a similar measurement of Mr. Dawes in 1851. (*Monthly Notices*, vol. xi. p. 52.)

M. Struve next proceeds to show that the observations of various astronomers anterior to the year 1838, afford unequivocal indications of the existence of the obscure ring. He remarks that the early observers generally make mention of a dark belt (termed the equatoreal belt) which was seen passing across the body of the planet *immediately contiguous* to the inner edge of the interior bright ring. J. Cassini showed in 1715, that from its slight curvature the equatoreal belt could not be situate close to the surface of the planet. The observations of Hadley, a few years later, would seem to establish almost beyond doubt the identity of the equatoreal belt with the obscure ring of modern astronomers. In 1723 he remarked that the dusky line which in 1720 he observed to accompany the inner edge of the ring across the disc, *continued close to the same, though the breadth of the ellipse had considerably increased since that time.* (*Phil. Trans.* 1723, No. 378.) He asserts, moreover, in reference to the hypothesis of the belt being produced by the shadow of the ring upon the planet, that when he considered the situation of the sun with respect to the ring and the planet, he found that the belt could not arise from such a cause.

As confirmatory of the same hypothesis, M. Struve remarks that the projection of the obscure ring upon the body of the planet agrees with the position assigned by the earlier observers to the equatoreal belt, and that no trace of the latter is visible upon the planet in the present day. He therefore arrives at the definitive conclusion that the obscure ring has existed around the planet at least since the time when telescopes were first constructed of sufficient optical power to exhibit the belts upon his disc.

The most serious objection which appears to M. Struve to offer itself to this hypothesis is founded on a remark made by Maraldi in 1715, to the effect that, during the disappearance of the ring in that year, the equatoreal belt continued still to be visible. If, indeed, that phenomenon be attributed to the projection of the obscure ring upon the body of the planet, it is difficult to imagine how it could have been perceptible on the occasion of the passage of the sun through the plane of the ring. M. Struve suggests as a probable explanation of this fact, that the obscure ring may be considerably thicker than the bright rings. The phenomenon, if sufficiently established, might perhaps serve to strengthen Mr. De la Rue's hypothesis of the obscure ring being situate in a different plane from that of the bright rings. It is so far confirmatory of the observation of Maraldi that M. Schmidt, of Bonn, perceived an obscure belt across the equator of the planet on the occasion of the passage of the sun through the plane of the ring in the year 1848 (*Ast. Nach.* No. 650). Future observations made during the disappearance of the rings can alone place the existence of the anomaly beyond all doubt.

The breadth of the space which separates the inner edge of the interior bright ring and the body of the planet, having exhibited a considerable discordance, as measured by himself in 1851, and by his father in 1826, M. Otto Struve was induced to institute a careful examination of the various measurements of preceding astronomers relative to the dimensions of the planet and the rings. The conclusion at which he arrived on this subject is somewhat curious. By a comparison of the micrometrical measures of Huyghens, Cassini, Bradley, Herschel, W. Struve, Encke and Galle, with the corresponding measures executed by himself, he found *that the inner edge of the interior bright ring is gradually approaching the body of the planet, while at the same time the total breadth of the two bright rings is constantly increasing.* It may be remarked, that the earlier measures employed in this comparison are founded upon certain assumptions relative to the effects of irradiation. They cannot, therefore, be regarded as altogether trustworthy.

In the concluding part of his memoir, M. Struve examines whether the two bright rings participate in an equal ratio in the enlargement of breadth which he found them to be constantly undergoing when considered as one appendage. He infers, from a comparison similar to that instituted in the previous case, *that, during the interval which elapsed between the observations of J. D. Cas-*

sini and those of Sir William Herschel, the breadth of the inner ring had increased in a more rapid ratio than that of the outer ring. The subsequent measures seem to indicate a reversal of this process.

The view of *Saturn*, given in the present Number, has been carefully drawn to scale and engraved by Mr. Warren De la Rue. The *principal* dimensions are taken from the *Nautical Almanac*, so far as they extended. Mr. De la Rue suggests that the data should include the major and minor semi-axes of the ball of the planet; the semi-axis minor of the dark ring; and the semi-axes of the divisions, according to the best authorities. His opinion is, that the large and fine telescopes which are now in use would enable a careful observer with a practised eye to fill up, and even to correct, the representation of micrometrical measures, and that, if the data for delineating *Jupiter* were given, his figure might probably be corrected by comparing the representation with the planet itself, seen under the most favourable circumstances.

Mr. De la Rue proposes to unite in one sketch the phenomena of *Saturn*, which have been seen by MM. Dawes, Lassell, and himself.

Professor Chevallier finds from a comparison between his ephemeris of *Hygeia* and observation that the errors alter very slowly; they are as follows :—

	Computed—Observed. R.A.	N.P.D.	
1852.		°	
Oct. 24	−30·54	+27·6	
Nov. 2	30·87	31·5	Cambridge Meridian
30	32·08	48·1	
Dec. 2	−31·96	+50·9	

He has, therefore, thought it better, " instead of attempting to correct the elements from partial observations, to compute accurately the planet's places from the same elements, during the entire period of its present appearance."

The ephemeris has been printed by the Professor, who has had the kindness to send copies for distribution to the Royal Astronomical Society.

BIELA'S COMET. *Ast. Nach.* No. 836, p. 327.

The following and southern portion of Biela's comet was observed at Cambridge on Sept. 8, 16, 21, 1852. Professor Challis finds that the Roman observations of August 25, 27, 28, and Sept. 1, as well as all the Cambridge observations, belong to the *southern* and *following* portion. The observation of Professor Secchi on Sept. 15, and the Berlin observations of Sept. 17, belong

to the *preceding* and *northern* part. Professor Challis further remarks, that the observations at Rome on Sept. 19 and 20, and at Berlin Sept. 22, confirm the above conclusions. Professor Secchi says, that the following head is the brighter of the two, and this probably corresponds to the larger head in the apparition of 1846.

The publication of *Cometic Orbits, with copious Notes and Addenda, compiled and edited by* Edward J. Cooper, 8vo. pp. 196, Dublin, 1852, was announced, but, unluckily, a copy for reference was not in the room. This work has already been alluded to by Mr. Graham.*

It is well known to the cultivators of cometic astronomy, that Dr. Galle appended a corrected list of all the computed cometary orbits which had been observed before June 1847, to Professor Encke's edition of Olbers' celebrated *Abhandlung*. But this work, notwithstanding its great accuracy and research, is in a considerable degree inaccessible to many English astronomers on account of its language; and the brevity of the notes implies a readier access to a large library than is usually the case. Mr. Cooper and his coadjutor, Mr. Graham, speak in the highest terms of Dr. Galle's labours, and have availed themselves of his researches: when they had not the means of consulting the reference, they have relied upon his authority in preference to any other.

Mr. Cooper's catalogue of comets extends to IV, 1851, which passed its perihelion Sept. 30·8, mean Greenwich time. The dates in the volume are *all* reduced to Greenwich time.

As to the authorities adopted for the construction of this catalogue, Mr. Cooper says:—"In the selection of orbits of any comet, where a great number have been computed, I had the advantage of Mr. Graham's assistance. Our adopted rules were to prefer those calculated upon the largest arc, having regard to the soundness of the data, and the reputation of the computer; to reject parabolic and hyberbolic elements, where no doubt or but little existed as to the ellipticity of the orbit, and to preserve those elements of any one comet that agreed best with one another." Following up a remark made by Sir J. Herschel, *Outlines of Astronomy*, p. 377, that "a plane of motion nearly coincident with the ecliptic and a periodical return are circumstances eminently favourable to direct revolution in the cometary, as they are decisive among the planetary orbits" (an idea which had, indeed, occurred some years ago to Mr. Cooper himself), Mr. Cooper "thought it desirable to investigate whether or not any other apparent analogy might be deducible from the heliocentric longitudes of perihelions and ascending nodes." To ascertain this, the comets having ecliptic orbits were arranged in tables as follows:—

* Vol. xii. p. 34.

Table I.—Ecliptic orbits in the order of their inclinations.
Table II.—Comets whose revolutions are under 500 years, arranged in quadrants of the longitude of perihelion, commencing with from longitude 45° to 135°, and in the order of their inclination in each quadrant.
Table III.—Planets arranged in quadrants, as in Table II.

From a consideration of these tables, Mr. Cooper is induced to believe that to Sir J. Herschel's hypothesis "that direct motion or a small inclination are favourable indications of a periodic comet," we may add "a longitude of the perihelion and of the ascending node between 45° and 135°."

The notes appended by Mr. Cooper are very full and very instructive, and intelligible: they form, indeed, an essential feature in the volume.

Astronomische Beobachtungen auf der Sternwarte zu Bonn, *aufgestelt und herausgegeben von* Dr. F. W. A. Argelander. *Zweiter Band,** 1^te^ *Abtheilung.* 4to. pp. 224. Bonn, 1852.

This work is a continuation of the zone observations of Professor Argelander, from Zone 205 to 321, and contains stars between 15° and 31° of south declination. It may thus be considered as a prolongation of Bessel's Zones to the south, as the previous volume was an extension to the north. There are auxiliary tables, after Bessel's method, for the reduction of these observed places to the mean places for 1850, Jan. 0; but as the stars in these zones are observed so near the horizon, Professor Argelander has added a peculiar set of auxiliary tables as an appendix to the zones, by which the labour of reduction is considerably reduced.† Professor Argelander proposes to bring out these Southern Zones in three parts (*Monthly Notices*, vol. xii. p. 25), and hopes that they will give additional accuracy to past and future planetary and cometary observations made out of the meridian.

Professor Argelander remarks that the declinations of the southern stars in the B. A. Catalogue appear to be a mean of Taylor and Brisbane, and that Brisbane's catalogue sets the stars considerably too much to the south. It is much to be regretted that Mr. Taylor's second catalogue (*Madras Observations*, vol. vi., Madras, 1844), appeared *after* the completion of the B. A. Catalogue. Mr. Baily had only Taylor's *partial* catalogues to refer to, which are affected by a systematic error in the division of the

* The first volume, Bonn. 1846, contains the zones up to 205, which include the stars between 45° and 80° of north declination. Bessel's zones from 15° south declination to 45° north declination are to be found in the volumes of the Königsberg Observations. Of the latter, the stars between 15° north and 15° south declination have been reduced and catalogued. *Catalogus Stellarum ex Zonis Regiomontanis, auctore* M. Weisse, 4to. pp. 284. Petropoli, 1846.

† *Hülfstafeln zu den Bonner südlichen Zonen*, 12mo. pp. 99. Bonn, 1852.

Madras circle. In Mr. Taylor's second catalogue this error is corrected.*

Annalen der K. K. Sternwarte in Wien, dritter Folge, erster und zweiter Band. 8vo. Wien, 1851–2.

The particular title of this book is, *Argelander's Zonen-Beobachtungen vom* 45° *bis* 80° *nördlicher Declination in mittleren Positionen für* 1842·0, *geordnet,* von Wilhelm Oeltzen.

This reduction of Argelander's northern zones was in part performed by M. Oeltzen before the autumn of 1850, when he was attached to the Vienna Observatory. Its early completion is due to M. Littrow, the director of the Observatory, who relieved M. Oeltzen as much as possible from his ordinary work.

The catalogue contains 26,425 stars, arranged in order of right ascension, with the magnitudes, right ascension to hundredths of a second, and declination to tenths of a second. To each star is appended the zone and number which belong to it in Argelander's observations. There are no precessions given, but two tables by which they may be computed, and the reduction to another epoch readily effected. A short introduction of twenty pages (in German) explains M. Oeltzen's mode of procedure.

Logarithmorum VI. Decimalium Nova Tabula Berolinensis, &c. Auctore Carolo Bremiker. 8vo. Berolini, 1852.

This volume contains the logarithms of numbers and logarithmic sines, cosines, and tangents for every 10″ to six places of decimals. There is an introduction (82 pages) in Latin, explaining the use and construction of the tables. The work is very clearly printed, nicely arranged, and of a convenient size.

Elements and Ephemeris of Westphal's Comet (II. 1852), by August Sonntag.

The periodic time is 60·8 years. The ephemeris extends from 1852, Nov. 12, to 1853, Feb. 1. *Ast. Nach* No. 836, p. 322.

Elements of Psyche, compared with all the observations which could be collected, by M. Klinkerfues, of Göttingen Observatory. *Ast. Nach.* No. 831, p. 234.

* This valuable Catalogue is also published as an independent work: "A General Catalogue of the principal Fixed Stars [11,015], from Observations made at the Honourable the East India Company's Observatory at Madras in the years 1830–1843, by Thomas Glanville Taylor, &c." 4to. pp. 272. Madras, 1844.

Comparison by M. Brorsen of all the Observations of the Comet II. 1851, with his elliptic elements: published in No. 782, *Ast. Nach.* No. 827, p. 270.

Notice of the establishment of an observatory at Toulouse, and a particular account of a shooting star observed at Bourdeaux and Toulouse, with the elements of its trajectory, by M. F. Petit, Director of the Observatory of Toulouse. *Ast. Nach.* No. 834, p. 287.

Observations of Shooting Stars on the evening of the 9.10th of August, 1852, on board the Ville de Paris. By M. E. de Jonquières. (Letter to M. Arago.)

(*Comptes Rendus, Sept.* 13, 1852.)

These observations were made on the eastern coast of the island of Sardinia. The number of stars which appeared every hour amounted to about seventy. The purity of the atmosphere allowed the trace of each *fusée* to continue sufficiently long to be enabled to form a good estimate of its direction. All the stars appeared to diverge from the same point of the heavens. This point was situate between 35 (*Hev*) *Cassiop.* and 33 *α Persei*, but somewhat nearer the former than the latter of these two stars.

Note on the Means of diminishing the Oscillations produced at the Surface of Mercury in the vicinity of ordinary Roads, Railroads, and Manufactories, with the view of facilitating Astronomical Observations. By MM. Séguin and Mauvais.

(*Comptes Rendus, Oct.* 11, 1852.)

The experiments were made at Fontenay. No sensible amelioration was obtained by placing the vessel containing the mercury upon elastic bodies, such as bales composed of layers of caoutchouc piled above each other, empty balloons of caoutchouc, &c. &c. When the vessel, however, was *suspended* through the intervention of an elastic substance, the oscillations were in every instance diminished. When vulcanised caoutchouc was used for this purpose the oscillations were completely annihilated, it having been impossible to perceive the slightest trace of them when the reflected images of different objects were observed with a portable telescope of moderate magnifying power. In this case the vessel was suspended by a cord passing through the caoutchouc disposed in the form of a ring.

On the Periodic Return of the Solar Spots.

(*Comptes Rendus, Nov.* 8, 1852.)

M. Wolf, Director of the Observatory of Berne, mentions, in a letter to M. Arago, that he has recently been engaged in researches on the solar spots, and has arrived at some interesting conclusions on the subject. By a comparison of all the observations of the spots made from the epoch of their discovery down to the present time, he has discovered that the number visible upon the surface of the sun in the course of a year recurs at regular intervals of time. The mean duration of the period comprised between two maxima or minima he finds to be $11{\cdot}111 \pm 0{\cdot}038$ years, which, he says, agrees much better with the variations in declination of the magnetic needle than the corresponding period of $10\frac{1}{2}$ years assigned by M. Lamont. He has also ascertained, that the years during which the spots have been most numerous, have been also the driest and most fertile, agreeably to a remark of Sir William Herschel's.

Note sur la Disposition la plus favorable à donner aux Appareils destinées à atténuer les Vibrations de la Surface du Mercure, et sur les Moyens d'approprier les Appareils à l'Usage des Instrumens méridiens. Par M. Mauvais.

(*Comptes Rendus, Nov.* 15, 1852.)

M. Mauvais had previously communicated the details of a series of experiments, by which it was found that the oscillations of the mercury were almost completely extinguished when the vessel containing it was suspended by an elastic substance composed of vulcanised caoutchouc. In the present note he proposes the following mode of applying this principle to meridional observations.

In a horizontal plank four vertical bars are inserted, and to their extremities are attached an equal number of straps of caoutchouc, having their lower ends fixed in a circular horizontal plate destined to receive the vessel containing the mercury. In order to maintain the circular plate in a steady position, a rod is attached underneath at the centre of the plate, carrying at its extremity a heavy ball.

Note de M. Faye sur les dernières Resultats publiées par M. De Struve relativement à la 61^e du Cygne.

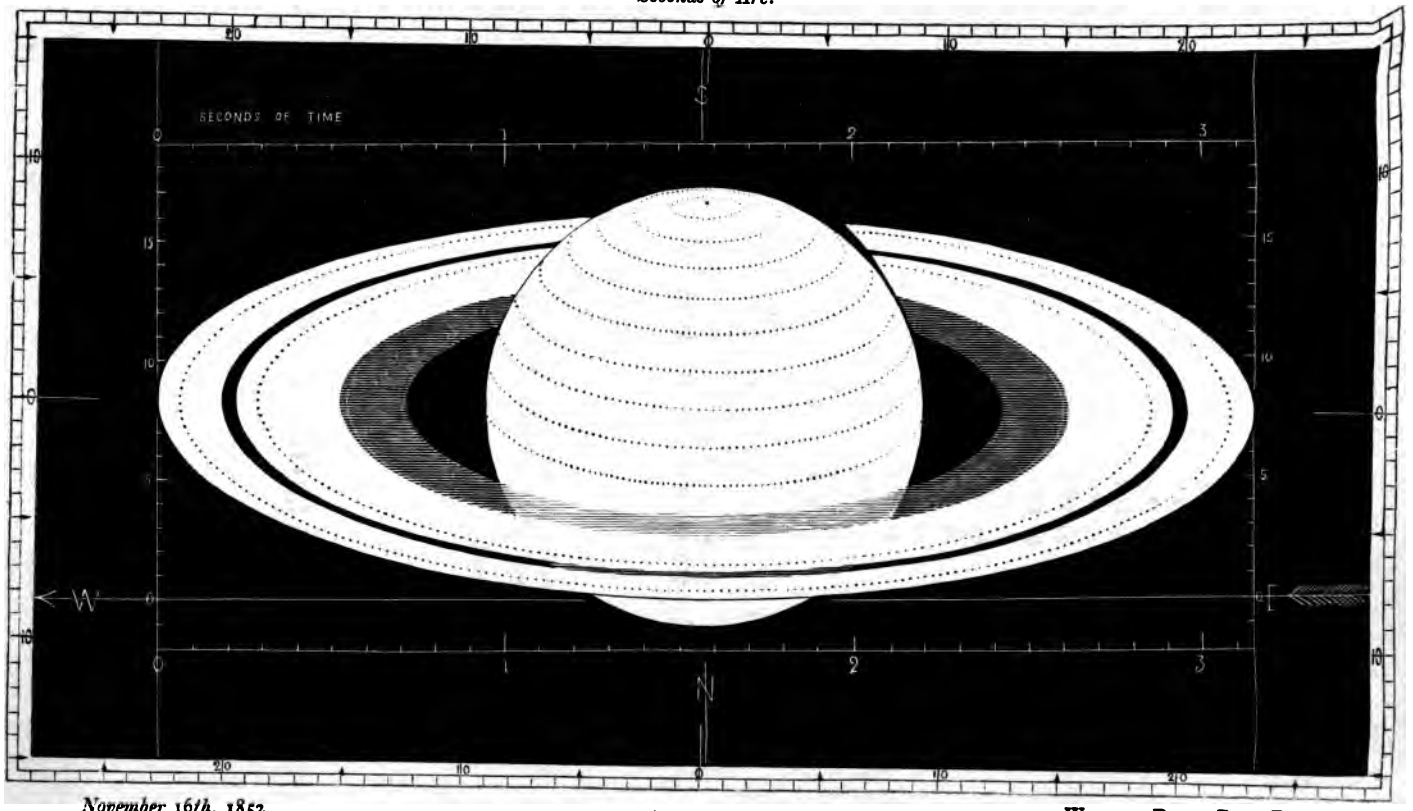
(*Comptes Rendus, Nov.* 22, 1852.)

In this note M. Faye calls attention to the fact that M. Struve, in his recently-published Dorpat Catalogue, has announced that, during the last hundred years, the relative motion of the components of this star has been sensibly uniform and rectilinear. This result essentially vitiates all the calculations that have been hitherto made respecting the orbit and mass of the star.

The instruments of the late Dr. Pearson are at present on sale; they may be viewed at Mr. Ross's, 2 Featherstone Buildings, Holborn. Among many valuable articles, the ***Azimuth and Altitude instrument,*** designed and partly constructed by Troughton, but finished and divided originally by Thomas Jones, and the ***Astronomical Telescope,*** $6\frac{1}{2}$ inches, ground by Tully, from glass presented by Guignand to the Astronomical Society, may be mentioned as remarkable instruments.

London:—Printed by G. Barclay, Castle St. Leicester Sq.

Seconds of Arc.



November 16th, 1852.

WARREN DE LA RUE, DEL.

ROYAL ASTRONOMICAL SOCIETY.

VOL. XIII. *Dec.* 10, 1852. No. 2.

Capt. W. H. SMYTH, V.P., in the Chair.

John Stanistreet, Esq., Liverpool, was balloted for, and duly elected a Fellow of the Society.

List of Variable Stars detected by Mr. Hind, with their Approximate Places for the year 1850.

No.	Mean R.A. 1850.	Mean N.P.D. 1850.	Magnitudes, Max. and Min.	Date of Discovery.
	h m s	° ′		
1	1 9 44	81 51.4	9 — 13	1851 Aug. 24
2	1 22 54	87 53.6	7.8 — 9.10	1850 Nov. 4
3	4 20 5	80 10.5	9 — 0	1850 — 14
4	4 50 50	82 6.3	9 — 12.13	1848 Aug. 24
5	6 58 20	67 4.1	7.8 — 11	1848 Oct. 11
6	7 34 2	66 12.2	9 — 0	1848 Feb. 25
7	7 40 18	65 53.8	9 — 0	1848 Mar. 4
8	8 35 22	70 24.9	8 — 10.11	1848 Feb. 1
9	8 45 44	86 22.0	8.9 — 13.14	1848 — 1
10	8 48 5	69 34.9	9.10 — 12	1850 Nov. 25
11	8 48 22	98 34.3	8.9 — 10.11	1851 Jan. 8
12	13 25 1	96 25.2	8 — 11	1852 Apr. 19
13	16 51 6	102 39.5	4.5 — 11.12	1848 — 27
14	20 2 51	104 42.4	9.10 — 0	1848 May 24
15	22 59 7	80 16.5	8.9 — 0	1848 — 30

Where dots are placed thus .... after the assigned magnitude at minimum, there is reason to suppose that the star may become fainter still. Nos. 6 and 7 have periods of about 296 and 292 days respectively (see *Ast. Nach.* No. 804). No. 8 varies in 9.02 days, or thereabouts (*Ast. Nach.* No. 804). No. 13 is the new star in *Ophiuchus* discovered by Mr. Hind in April 1848. (*Ast. Nach.* No. 832).

LUTETIA.

Elements. By Mr. W. Ellis, of the Durham Observatory, deduced from the Durham Observations of Dec. 27, 1852, and Jan. 2, and Jan. 11, 1853.

Epoch 1853, Jan. 1, 0^h Greenwich M.T.

	°	′	″	
Mean Anomaly	45	36	19·8	Mean Equinox of Epoch.
Long. Perihelion	3	41	41·9	
— Ascending Node	80	21	36·3	
Inclination	3	5	6·0	
Angle of Eccentricity	6	36	44·5	
Log *a*	0·4170508			

The middle observation is nearly satisfied by these elements.

An ephemeris by Mr. Ellis, founded on these elements, was sent at the same time; it extends from January 23 to March 6. Copies have been sent to those observers who attend to the small planets. (*See also Ast. Nach. No.* 847, p. 116.)

Letter from Mr. Lassell.

"*Valetta, 29th Dec.* 1852.

"Though I am almost afraid of *Saturn* having become an exhausted subject, yet, as he has since my last exhibited some change of phenomena, I will trouble you with another sketch of him, as he now appears, and a few more last words. Last night, after an interval of rather broken weather, the sky was unusually tranquil, and in the intervals of passing clouds I obtained views of the planets quite equal, if not superior, to any I have ever before seen. The power principally used was 650, speculum C in the tube, which afforded an image in respect of size, sharpness, and brilliancy, hitherto to my eye unexcelled.

"I have spoken highly of the definition of the other speculum, A; and certainly, under the best circumstances, I doubt if the *vision* would suffice to inform me which of the two specula was in the tube, so nearly are they alike; yet, in the average of many nights' observation, I think I incline to prefer C, from an impression of its greater uniformity of action. I doubt if I ever before produced consecutively two two-foot surfaces, so undistinguishable.

"For some time, as well as especially last night, I have noticed a peculiarity of form in the shadow of the ball of *Saturn* upon the ring, which I have endeavoured as faithfully as I can to depict in the accompanying drawing. Its curvature suddenly changes at the division of the rings, the part projected upon the ring B being almost, if not exactly, a right line. Would a *very small* amount of curvature (convexity) of the surface of ring B suffice to change the otherwise convex edge of the shadow into a right line? There cannot be a *large* amount of convexity of the surface, as it would then be optically manifest. A very similar appearance I noticed,

less distinctly, of the shadow of the ball on the western arm before opposition, mentioned in a former communication — probably it may be now more manifest, from the greater breadth of the shadow, or I have been led to notice it more. The 'spot' which I also spoke of being visible on the limb opposite to the shadow, has now no existence, and I think it may have been the first and last appearance of this angle of the shadow on the respective limbs.

"The shadow of the ring upon the ball has now become very obvious near the north pole, but exhibits a striking contrast to the shadow just spoken of in depth of shade; for while the latter is as perfectly black as anything can be, the former is only a dark grey, about the shade of the dark ring near the ansæ. In breadth, this shadow is now equal to rather more than half the breadth of ring A at that part which is contiguous to the shadow; and it appears to deepen in shade from its extreme northern edge to its junction with the ring. Probably the very oblique direction in which the shadow must fall on the rotundity of the planet may account for this paleness.

"I compared under these advantageous circumstances the two ends of the obscure ring, and indeed examined its general form, and could detect no want of symmetry or regularity of curve. In estimating, however, its similarity of form at the two ends, the judgment is somewhat disturbed by the projection of the shadow of the ball on the dark ring on the south following side. I noticed a difference of colour, the preceding end being a bluish-grey, while the following had a touch of yellow or brown in its greyness.

"The belts have altered considerably since my last letter. The division of the ruddy belt is much less plain. The first of the blue belts, which was very dark and decided, has nearly disappeared; the second, though fainter, is still traceable, while the small circular polar one remains as dark and visible as ever. I do not observe any recurring spot on the surface of the planet by which his rotation on his axis might be observed, though I have sometimes noticed that the shading on the ring B has not been the same at both ends. This, however, has not been sufficiently constant or distinct to serve any practical purpose.

"During the fine atmosphere of last night, I could not but be greatly struck with the beauty of the Saturnian picture afforded me. With the observing power of 650 nothing seems wanting to the precision of the image but the removal of the *minute flickerings* of the atmosphere, which with this power, and the aperture of my telescope, I do not recollect ever being unconscious of. The edge of the ring seems as hard and sharp as a metallic rim, and on a perfectly black ground in the quietest atmospherical moments. A sufficient proof of this, scarcely, however, to be duly estimated by those who have not been accustomed to look out for and observe the closest of *Saturn's* satellites, is, that *Mimas* was kept steadily in view, and watched up to his greatest western elongation, notwithstanding the splendour of the planet and widely-opened ring shining full in the field. I have introduced into the drawing

the approximate positions of the two nearest satellites as they appeared about $10^h\ 0^m$ Greenwich mean time. The greatest elongation of *Mimas* was estimated to occur at $10^h\ 4^m$, its distance from the end of the ring I estimated to be five-sixths of the sum of the breadths of rings A and B. This may serve as a guide to the possessor of any telescope powerful enough to give hopes of seeing this most minute and delicate object. *Enceladus* passed his inferior conjunction at about $8^h\ 12^m$ Greenwich mean time.

"This is really a splendid climate. There is now (at Christmas) just that amount of cool freshness in the air which makes exercise agreeable, and the valleys putting on their mantle of green give even the rock of Malta a fertile aspect. The enjoyment a walk affords in this transparent atmosphere, under this glorious sun, giving its warm colours to the architecture, and all manner of tints to the crystal waves as they break upon the shore, is more than my eloquence suffices adequately to convey to you.

"We all feel we have rather too much to do. People here think I am a gentleman of fortune, who has brought a large telescope here 'for his amusement!' How different from the fact: I call it real labour, and shall rejoice when my emancipation comes."

Extract of a Letter from Mr. Lassell.

"*Valetta, 10th Jan. 1853.*

"I have now to hand you the results of my observations upon *Neptune* and his satellite, in which you will see what I think of the probability of finding other satellites. With *Uranus* I have not quite done yet, though I am pretty well settled in the conclusion that he has no more satellites visible with my means than the four, so that my discoveries in this way are likely to be more negative than positive. However, I think it is high time that *Uranus's* establishment should be reduced. He has been luxuriating these many years with a retinue that I really believe does not belong to him, and, therefore, he must be cut down to four attendants, until some astronomer arises rich enough to present him with some more.

"We have the weather rather more broken now, but yet, I think, more starlight than occurs in the best seasons in England — quite as much as I have health and strength enough to use. We are astonished and delighted with the wonderful display of clusters and nebulæ a little below the equator, in the neighbourhood of *Orion*. One might be entertained and gratified by the hour, just to take the telescope in hand, and sweep slowly over the milky way east of that constellation. The full light of this telescope, charged with a power of from 160 to 200, and a field of about 20′ in diameter, with its precision of image and absence (with these powers) of sensible atmospheric disturbance, affords such pictures of the sidereal heavens as I cannot at all adequately describe, and as call forth continually from the observer involuntary exclamations of delight and astonishment."

Observations of Neptune and his Satellite.

1852.	G.M.T. h m	Position. ° ′	G.M.T. h m	Dist. ″	Reference Letter.
Oct. 28	7 54	37 6	8 8	16·62	(*a*)
29	6 43	48 36	6 56	13·19	(*b*)
30	7 17	159 est^{d}.			(*c*)
31	6 55	215 57	7 9	16·23	(*d*)
Nov. 1	6 20	223 32	6 39	12·77	(*e*)
2	7 7	9 13	7 23	4·45	(*f*)
3	6 8	37 15	6 29	15·78	(*g*)
4	6 21	49 29	6 34	11·40	(*h*)
5	7 0	177 0	7 0	4	(*i*)
6	6 16	215 42	6 32	16·57	(*k*)
9	6 26	38 34	6 40	17·20	(*l*)
10	6 41	53 52	6 51	9·60	(*m*)
11	6 15	197 53	6 41	5·26	(*n*)
12	6 2	216 36	6 12	16·81	(*o*)
15	6 4	40 57	6 21	17·42	(*p*)
16	5 58	57 17	6 15	7·08	(*q*)
17	6 13	205 12	6 25	7·64	(*r*)
18	6 2	217 59	6 16	16·99	(*s*)
19	6 4	232 51	6 15	6·69	(*t*)
20	5 47	26 28	6 2	10·93	(*u*)
26	5 37	31 46	5 50	11·34	(*v*)
29	6 6	207 18	6 16	11·36	(*w*)
Dec. 8	5 23	33 57	5 38	13·09	(*x*)
9	5 43	45 52	5 52	13·72	(*y*)
27	5 0	51 34	5 7	10·82	(*z*)

"(*a*). Sky hazy. No illumination of the field used but that given by the moon-lit sky—itself rather too great. Yet the satellite was perfectly easy, and the measures taken with great comfort and confidence. The planet was well defined and round.

"(*b*). Planet round and well defined. Satellite bright. The rising moon rendered artificial illumination unnecessary.

"(*c*). On turning the telescope on the planet, he appeared as usual, attended by his satellite near its greatest elongation; and if I had not *expected* to find the satellite near its closest appulse to the planet, I might have proceeded to measure its position and distance. I did, indeed, apply the micrometer, and found the distance about 2″ more than the satellite's greatest elongation. I mention the fact, as one of the *very rare* instances in which a star of the identical magnitude is found so near the place of a satellite as to be temporarily mistaken for it; but even here, applying the micrometer at once verified the illusion.

"I proceeded to look more carefully for the real satellite, with the usual measuring power of 760, but was for a long time disappointed, and feared I must give up the search. At length I took off the micrometer and applied Dollond's polycratic wheel of single convex lenses, powers 222, 479, 628, 805, 1292, and 1917, changing the powers successively from the second or third, but failed to get a steady glimpse of the satellite until I got to the highest, 1917, with which I saw it pretty steadily and well, though the planet itself with this power was like a ball of wool. I estimated the distance of the satellite about three-fourths of the

always under less advantageous circumstances than on the night of January 9. On former evenings, the contour only of the globe appeared, by momentary glimpses, to penetrate through the obscure ring; whereas, on January 9th, the appearance was such as to convey the idea that it was not merely the outline, but the whole surface traversed by the dark ring, which was visible through it.

"From observations last autumn, I had considered it very probable that the continuation of the outline of the ball through the obscure ring was an optical illusion, arising from the impression made upon the eye by the brightness of the ball in comparison with that of the ring; and though I conceive this explanation should not be hastily set aside, the results of the favourable examination on January 9th have rather shaken my confidence in it: the dark ring on this night certainly appeared to possess a *partial transparency*, which would account for the contour of the globe being perceptible up to the interior bright ring. It was impossible not to remark the much lighter (greyish) shade presented by the projection of the obscure ring upon the ball, as compared with the black shadow of the latter upon the further side of the bright rings.

"*South Villa Observatory, Jan.* 13, 1853."

Some Remarks on the probable present Condition of the Planets Jupiter and Saturn, in reference to Temperature, &c. By Mr. Nasmyth.

"The remarkable appearances which characterise the aspect of the planets *Jupiter* and *Saturn*, as revealed by the aid of very powerful and excellent telescopes, have induced some reflections on the subject of their probable present condition as to temperature, that with a view to excite more special and careful observation of the phenomenon in question, and promote some discussion on this interesting subject, I have been tempted to hazard the following remarks, which may perhaps prove acceptable to some of the members of the Royal Astronomical Society.

"In a former communication, in reference to the structure and condition of the lunar surface, I made some remarks on the principle which, as it appears to me, gives the law to the comparative rate of cooling of the masses of the planets, namely, that while the heat-retaining quality was due to the mass of the planet, the heat-dispensing quality was due or governed by its surface; and as the former increases as the cube of the diameter of the planet, while the latter increases only as the square of its diameter, we thus find that the length of time which would be required by such enormous planets as *Jupiter* and *Saturn* to cool down from their original molten and incandescent condition to such a temperature as would be fitted to permit their oceanic matter to permanently descend and rest upon their surface, would be vastly greater than in the case of such a planet as the earth.

"Adopting the results which geological research has so clearly established as respects the original molten condition of the earth, as our guide to a knowledge of the condition of all the other planets, it appears to me that we may in this way be led to some very remarkable and interesting conclusions in reference to the probable present condition of such enormous planets as *Jupiter* and *Saturn*, as may tend to explain certain phenomena in respect to their aspect.

"Assuming as established the original molten condition of the earth, and going very far back into the remote and formative periods of the earth's geological history, we may find glimpses of the cause of those tremendous deluges, of which geological phenomena afford such striking evidence, and by whose peculiar dissolving and disintegrating action on the igneous formations which at that early period of the earth's history must have formed the only material of its crust, we may in that respect obtain some insight into the source whence the material which formed the first sedimentary strata was derived. If we only carry our minds back to that early period of the earth's geological history, where the temperature of its surface was so high as that no water in its *fluid* form could rest upon it, and follow its condition from such non-oceanic state to that period at which, by reason of the comparatively cooled-down condition of its surface, it began to be visited by partial and transient descents of the ocean, which had till then existed only in the form of a vast vapour envelope to the earth, we shall find in such considerations, not only the most sublime subject of reflection in reference to the primitive condition of the earth, but also, as it appears to me, a very legitimate basis on which to rest our speculations in regard to the probable present condition of *Jupiter* and *Saturn*,—both of which great planets, I strongly incline to consider, for the reasons before stated, are yet in so hot a condition, as not only not to permit of the permanent descent of their oceanic matter, but to cause such to exist suspended as a vast vapour envelope, subject to incessant disturbances by reason of the abortive attempts which such vapour envelope may make in temporary and partial descents upon the hissing-hot surface of the planet.

"Recurring again to this early period of the earth's geological history, when it was surrounded with a vast vapour envelope, consisting of all the water which now forms the ocean, the exterior portion of this vapour envelope must, by reason of the radiation of its heat into space, have been continually descending in the form of deluges of hot water upon the red-hot surface of the earth. Such an action as this must have produced atmospheric commotions of the most fearful character; and towards the latter days of this state of things, when considerable portions of what was afterwards to form our ocean came down in torrents of water upon the then thin solid crust of the earth, the sudden contraction which such transient visits of the ocean must have produced on the crust of the earth would be followed by tremendous contortions of its surface, and belchings forth of the yet molten matter beneath, such

as yields legitimate material for the imagination, and the most sublime subject for reflection. The extraordinary contortions and confusion into which the more primitive sedimentary strata, such as the gneiss, schist, and mica slate, are found to have in so very indistinct degree, shadow forth the state of things which must have existed during that period, when the ocean held a very disputed residence on the surface of the earth.

" Could the surface of the earth have been viewed at that era of its geological history from such a distance as the planet *Mars*, I doubt not it would have yielded an aspect in no very dissimilar respect to that which we now observe in the case of *Jupiter:* namely, that while the actual body of the earth would have been hid by the vast vapour envelope then surrounding it, the tremendous convulsions going on within this veil would have been indicated by streaks and disruptions in the belts; and those, again, mottled over with markings, such as we observe in the case of the entire surface of *Jupiter:* and by reason of the belchings forth of the monstrous volcanoes which at that period must have been so tremendously active on the earth, the vapour envelope would be most probably marked here and there with just such dingy and black-and-white patches, as form so remarkable features about the equatoreal region of *Jupiter*—probably the result of volcanic matter, such as ashes, &c.—which the several volcanoes may from time to time vomit forth, and send so far up into its cloudy atmosphere as to appear on the exteriors, and so cause those remarkable features which so often manifest themselves on the exterior of his vapour envelope; for I doubt if we have ever seen the body of *Jupiter* yet, which will probably remain veiled from mortal eyes for countless ages to come, or until he be so cooled down as to permit of a permanent residuum on his surface of his ocean that is to be.

" In applying these views to *Saturn*, it occurs to me that we may in that respect obtain some glimpse into the nature of those causes which have induced, and are now apparently inducing, those changes in respect to the aspect of his rings, which have, more especially of late, attracted so much attention. If he also be so hot, that his future ocean is also suspended as a vast vapour envelope around him, it is possible, I conceive, that some portion of this vapour may migrate, by reason of the peculiar electrical conditions which it is probable his rings may be in, in respect to the body of the planet; and that such migration of vapour in an intensely frozen state, as it must be in such situation, may not only appear from time to time, as the present *phantom ring* does, but also encrust the inner portion of the inner old ring with such coatings of hoar-frost, like crystallised water, as to cause the remarkable whiteness which so peculiarly distinguishes that portion of his rings. Such, in fact, are the extraordinary phenomena of this planet, that one is led to hazard a conjecture or two on the subject; and, I trust, such as I have now the pleasure to offer may have a kind reception from the Royal Astronomical Society.

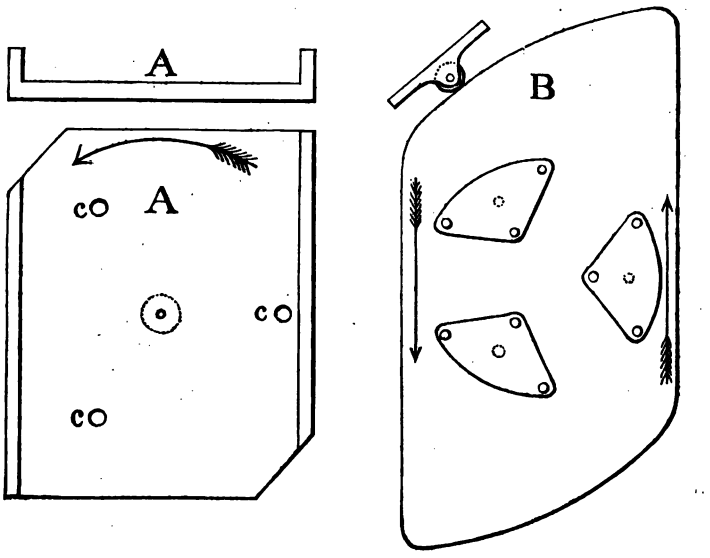
"*January* 13, 1853."

Description of an Improvement in his Machine for Polishing Specula. By Mr. Lassell.

"During a pretty extensive use of the polishing machine described in the eighteenth volume of our *Memoirs*, and while seeking especially a higher excellence of defining power than I had up to that time attained, I noticed that the first progress of incipient polish manifested an *annular* structure, and, therefore, gave unequivocal evidence of some want of uniform action over the whole surface of the speculum.

"In some instances I have noticed the lustre first appearing in a central spot of perhaps a tenth part in diameter of the diameter of the speculum, and surrounded by six or eight annuli of partially polished surface, alternating up to the edge of the speculum with annuli of less reflective surface. As the polishing process advanced, this inequality of lustre became less evident, and when completed no trace of it was apparent. But, of course, the inference was inevitable, that the same irregularity must exist, though not manifest to the eye; and I forthwith became persuaded that it would be necessary, if possible, to introduce some transverse action, in order to wipe out, as it were, these rings, which, it appeared, the purely circular motion of the quick and slow-moving cranks had a tendency to produce.

"This end has been accomplished with, I think, perfect success, by a simple apparatus introduced between the upper surfaces of the vertical spindle and the speculum itself, leaving all the motions of the upper cranks and other parts of the machine untouched.



"In the annexed figures, A A are two views of a cast-iron plate

about half-inch thick, which, by the three holes C, drops upon three studs screwed into the upper surface of the wheel of the vertical spindle carrying the speculum. B is a plate about the same thickness as A, placed on A's surface, and easily sliding within the ribs cast on the edges. On the upper surface of B, the speculum rests on its plates. The ends of B are curved, as in the figure, and as the plates A and B revolve, carried by the vertical spindle, the curved ends of B are *each* brought, *once* in a revolution, in contact with a pulley or roller fixed in the upright wall-plate of the machine, and adjustable as to proximity. The contact of the roller forces B to slide, smoothly and regularly, athwart A, producing the effect of a regulating or correcting cross stroke, amidst all the circular ones, and quite annihilating all ring-like tendency. The amount of lateral thrust necessary is not great, and I have used from one inch with a nine-inch speculum to one and a half or two inches with a two-foot speculum. With a shorter proportion of focus, less than this would be sufficient; for its effect is to reduce in some degree the parabolic tendency of the machine.

"The improvement of definition, as well as of certainty in the operation, which this simple arrangement produces, is most striking, and gives, I would almost venture to say, the last touch of perfection to the machine.

"*Valetta, 24th November*, 1852."

On the Figuring of Specula. By Warren De La Rue, Ph.D., F.R.S.

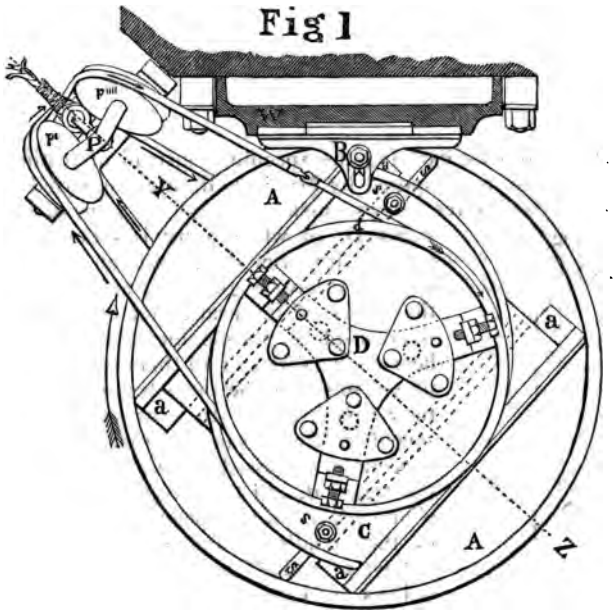
It has been said of the reflecting telescope, that "no one, competent to form a judgment on the subject, was ever satisfied with such an instrument, excepting the man who made it." This remark, inasmuch as it may be referred to the definition of a fixed star, is so true, that, in my own case, I include the maker of the instrument as also one of the dissatisfied parties. At the same time it must, however, be stated, that I usually succeed in producing thirteen-inch mirrors, which define the planets *Mercury*, *Venus*, *Mars*, *Jupiter*, *Saturn*, and *Uranus*, with the surface of the moon, and particularly its *bright* limb, in a manner rarely equalled, and never surpassed, by any of the refractors which I have yet had an opportunity of looking through. I am, however, free to confess, that they defined a fixed star much more satisfactorily than my best mirrors.* I am, therefore, inclined to believe that it requires a far more accurate figure in order to show a *star free from rays*, in the case of a reflector, than in that of a refractor. Difficult as the attainment of this object has hitherto proved, recent results of my own, as well as those obtained by Mr. Lassell by means of his "transverse-motion," lead me to hope that the obstacles in the way of that attainment are not insurmountable. The design of the present communication is, indeed, to bring under the notice of the Society two additions to Mr. Lassell's polishing-machine; which,

* I have seen the components of *ω Leonis* neatly separated by the mirrors to which I allude.

in conjunction with the "transverse-motion," have enabled me to produce figures showing as discs the components of such stars as *γ Leonis* and *α Geminorum*, whenever the state of the atmosphere admits of the use of magnifying powers from 500 to 700. With a low power (150) a bright star is surrounded by rays, so faint, however, that they appear reddish in comparison with the intense brilliancy of the nucleus.

By referring to Mr. Lassell's description of his "transverse-motion," it will be perceived that the cross-stroke occurs always in the direction of the same diameter of the speculum; a result which it appeared to me very desirable to obviate, inasmuch as the recurrence of the change of excentricity at precisely the same epoch must have some tendency, however small, to produce an irregularity of figure. In adopting the "transverse-motion," I therefore superadded a very simple contrivance for causing the speculum to rotate independently of the cam-plate. This arrangement is delineated in figures 1, 2, and 3, which are drawn to a scale of one inch to the foot, and also represent so much of the machine as is necessary to a clear comprehension of its position in relation to the other parts. Figure 1 is a plan; figure 2 a sectional elevation through the dotted line Y Z of figure 1; and figure 3 a back-view of a swinging pulley-frame.

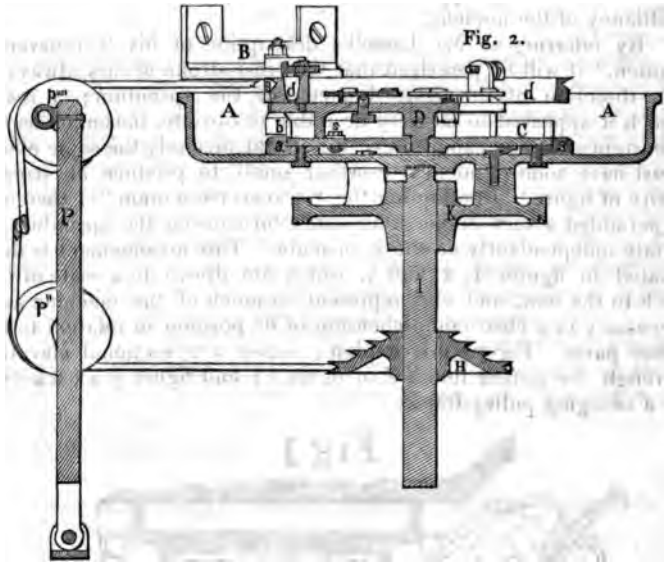
Fig 1



I is the lower spindle of the machine, on which is keyed the worm-wheel K, to three projections from which is screwed the pan A. On the bottom of the pan are screwed two guides, *a*, be-

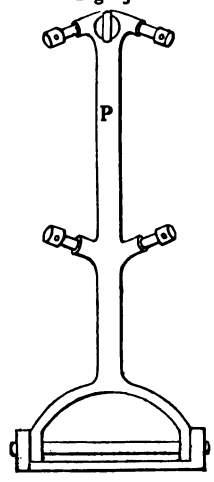
tween which the cam-plate, *c*, slides, when acted upon by the roller *b*, in consequence of its own rotation in the direction of the arrow. The roller *b* turns freely on a spindle, adjustable, for the purpose of altering the throw, in the slotted plate B, which is fastened to the wall-plate W, shown in figure 1, but not in figure 2.

Fig. 2.



The tripods for carrying the speculum, instead of resting upon the cam-plate, as proposed by Mr. Lassell, are supported by the capped ends of three screws passing through the arms of a tripod-stand D, to which arms is attached a light wrought-iron rim *d*, grooved on its periphery so as to receive a gut. An axis projects from the centre of the arms, and enters a hole in the cam-plate: its shoulder does not, however, bear on its surface; so that the arms are supported solely by the three legs projecting from their under-side. In order to prevent the speculum from being carried round by the cam-plate, a crossed endless gut is led round the rigger *d*, and then over the guide-pulleys *p'* and *p''*, round the speed-rigger H, keyed on to the spindle I, and then over the guide-pulleys *p'''* and *p''''*, back to *d*. The four guide-pulleys are carried by the swinging-frame P; and to an eye at the back of this frame is attached one end of a rope, which passes over a pulley, and carries a tightening weight at its other end. In consequence of this arrangement,

Fig. 3.



the speculum, instead of rotating in the same time as the cam-plate, has, on the contrary, a slower motion, though revolving in the same direction.* The cross-strokes can, therefore, recur along the same diameter of the speculum only after many revolutions, on account of the ratio of the diameters of the two riggers *d* and H; which, for the greatest diameter of the latter (that hitherto used) is as 1·69 : 1; the cam-plate rotating 169 times, while the speculum rotates 100 times.

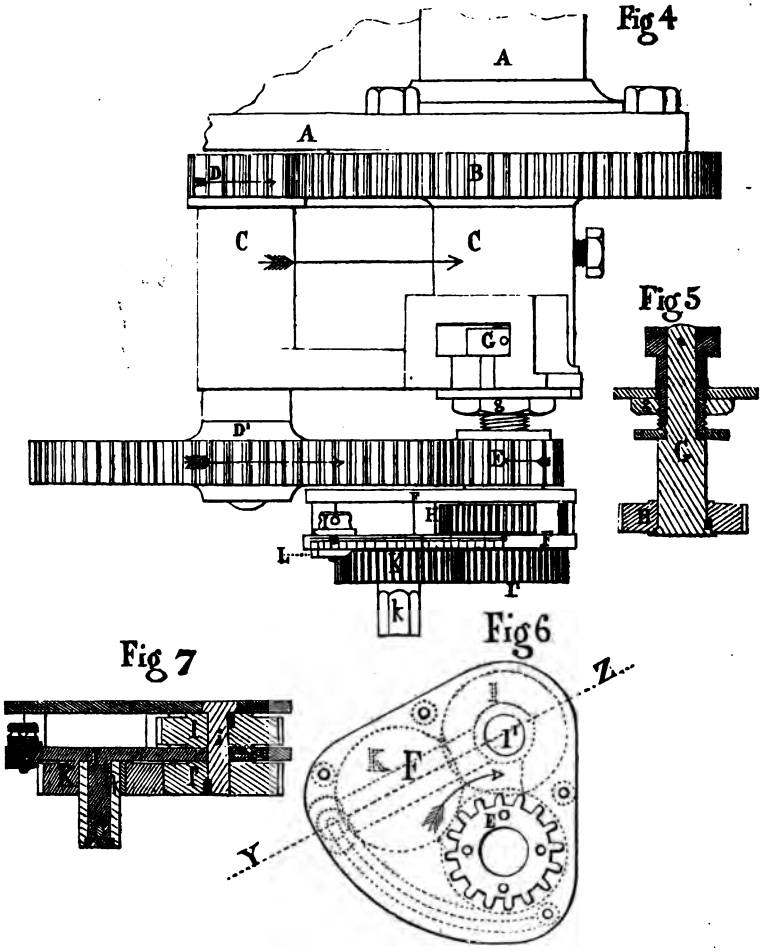
The swinging-frame P is so placed, that its pull is perpendicular to the guides *a*, when the cam-plate has completed its stroke; the tightening-weight, therefore, cannot then move it in any direction. As the pan continues its rotation the weight would, however, pull the cam-plate towards its rim, and increase the throw beyond that given by the roller *b*, unless prevented. To avoid such an occurrence there are two stop-bars, S, adjustable beyond the cam-plate to suit the various throws which may be given to it; namely, from 0 to 2·2 inches on each side of the centre. Each stop passes through a fixed guide at one end and a screwed-staple or eye, *s*, at the other; by drawing this eye tight by means of the nut *s*, the stop is held securely in its place.

None who have used a Lassell's polishing-machine can have failed to be struck with the uncertainty and irregularity of the motion of the polisher around its axis, the quick-crank pin; and to have experienced a strong desire to have this motion as much under control as all the other movements of the machine. The polisher, being quite free to rotate, usually zigzags in one or other direction; but seldom rotates smoothly, and certainly never with any approach to a constant velocity-ratio, however attentive the operator may be in regard to the due distribution of rouge and water, for the purpose of rendering the resistance as uniform as possible. This irregular action did not escape the observation of Mr. Lassell, as he some time since proposed a plan for driving the polisher by means of a gut or band from a pulley working on an axis affixed to a horizontal swinging frame, which, moving to and fro as the polisher was carried round, maintained a constant radial distance between the polisher and its driver. Mr. James Nasmyth, in describing this arrangement to me, in a letter dated October 9th, 1850, proposed another plan. It consisted in fixing a series of long pegs around the periphery of the polisher, and placing a stationary stop in such a position that one peg after another would come in contact with it, each time the polisher was swung towards it, thus causing the polisher to move through a small arc at every throw. After having tried several schemes with indifferent success, I devised the apparatus presently to be described, which is so simple, that I am only surprised it did not occur to me from the first.

Figures 4, 5, 6, and 7, drawn to a scale of three inches to the

* It is my intention to make a slight addition to the swinging-frame, in order to allow of an open gut being used, so as to have the facility of driving the speculum in the contrary direction also.

foot, are different views of a new quick-crank in substitution of that employed by Mr. Lassell. Figure 4 is an elevation of it, and of as much of the other parts of the machine as is required for the comprehension of the position of the new crank in respect to them. Figure 5 is a section of the crank-pin, and a fixed wheel, which is keyed thereon. Figure 6 is a plan of the new crank removed from the pin shown at figure 5; and figure 7 a sectional elevation of it, through the dotted line Y Z of figure 6.



A is the upper fixed bracket screwed to the wall-plate, and carrying the upper spindle of the machine. On this spindle is fixed the train-bearing arm C C, which revolves in the direction of the arrow. B is a wheel of 75 teeth, screwed fast to the fixed

bracket A, and geering with the pinion D of 16 teeth. The pinion D, and the wheel D′ of 75 teeth, are keyed on to a common spindle, which rotates in the arm C. The wheel D′ geers into the pinion of 16 teeth E, to which is fastened the triangular crank-frame F, composed of two wrought-iron plates, kept apart by four pillars, which are rivetted into them. The pinion E, and, consequently, the frame F, rotates freely on the spindle G, with a velocity ratio in respect to that of the arm C, represented by $\frac{75 \times 75}{16 \times 16} : 1$. Hence it would produce 21·97265 hypocyclic loops for one revolution of the upper spindle; on the supposition, however, that the lower spindle I, fig. 2, remains at rest: but as motion is given to this usually in a contrary direction by means of a gut working in one or other of the grooves of a pair of speed-pulleys (one on the upper and one on the lower worm-shaft), the number of loops varies with the velocity with, and the direction in, which it is driven.

The crank-pin G, which it will be remembered, from Mr. Lassell's description of his machine, may be adjusted so as to be either concentric with the upper spindle or at any degree of excentricity within the range of the machine, namely, five inches, is seen best in the sectional view, fig. 5. It is composed of the spindle proper and a hollow socket, into which this is held by a removable pin; the lower part of the socket being screwed, and carrying the nut *g*, surmounted by a washer: between the shoulder of the spindle proper and the end of the screwed socket is inserted a washer, so as to form a good bearing surface for the pinion E. On the lower end of the spindle G is keyed the wheel H, of 52 teeth. I and I′ are two wheels, each of 52 teeth, keyed on to a spindle *i*, which rotates freely in the rectangular crank-frame F; the wheel I geering in the fixed wheel H, receives a rotatory motion as the crank-frame carries it round it; I′, fixed on the same spindle as I, geers into the wheel K, having also 52 teeth: this is fastened on the hollow steel spindle *k*, which rotates freely on the spindle L, projecting from, and being a part of, the radial arm L. L is an arm moving on the spindle *i*, like a rule-joint, and adjustable by means of the circular slot at a position either concentric with the crank-pin G, or at any excentricity up to two and a half inches, the lower plate of the crank-frame being pierced with that view to an extent indicated by the dotted exterior outline of the arm L, the inner circular arc, the exterior outline of the wheel H, and the radial line joining it (see fig. 6). The screw *l* serves to hold the arm L firmly in its place. The effect of the epicyclic train H I, I′ K, the teeth of each wheel being equal in numbers, is to annihilate so much of the rotary motion of the spindle *k* as arises from its revolution around G, and hence to produce parallelism in respect to the arm C C; it, however, continues to rotate relatively to a fixed point of the machine once for every revolution of the main arm C C. The lower part of the spindle *k* is made hexagonal, in order to prevent the polisher from rotating without control; it is, however, quite at liberty to move up and down, so as to follow the

curvature of the speculum. As the rotation of the spindle *k* is compensated in regard to the arm C C, it follows that any line of the polisher parallel with it always retains this parallelism, and, consequently, that every point on its surface describes the same number of hypocyclic loops, in round numbers 22; but while the loops described by the centre are generally nearly circular, those near the periphery are much elongated.

If the polisher were made to maintain its parallelism with a fixed line in a space, and the speculum remained at rest, then every point of the polisher's surface would describe precisely the same figure. Such parallelism might be obtained sufficiently near, for practical purposes, by making the number of I''s teeth any multiple of 21, and those of K any multiple of 22; for instance, by giving to I' 63 teeth, and to K 66, because one rotation in twenty-two of the crank-spindle *k* is compensated by the revolution in the opposite direction of the arm C C, which carries the whole system.

These numbers would not, however, suit in the case of the speculum revolving, which it always does, but must be calculated for each velocity of the speculum; taking also into account the direction of its motion in relation to the arm C C.

I have not at present a train to produce an *approximate* parallelism (*absolute* parallelism would not be desirable) in relation to a diameter of the speculum; but I have two, besides that of equal wheels already described, either of which produces such an amount of rotation in the polisher as I have observed it take up when left uncontrolled, namely, No. 2, in which H has 52 teeth, I 52, I' 54, and K 50: it is, therefore, represented by the fraction $\frac{52 \times 54}{52 \times 50} = 1{\cdot}08$. When governed by this train the polisher rotates in relation to the fixed frame of the machine 2·76 times for one revolution of the arm, and in the same direction; hence, in relation to the arm, it rotates in the ratio of 1·76 : 1, and describes an epicycloid of two loops, or, more exactly, fifty-eight loops for thirty-three revolutions of the arm. Twenty-five hypocyclic loops, due to the excentricity of the spindle *k*, are distributed around this curve by the periphery of the polisher, whereas its centre distributes them around a circle, and produces a hypocycloid of twenty-two loops, precisely as is done by train No. 1; but it moves through 1·14 revolution while the periphery rotates once.

No. 3 train, in which K has 52 teeth, I 52, I' 51, and K 53; it is, therefore, represented by the fraction $\frac{52 \times 51}{52 \times 53} = \frac{51}{53} = 0{\cdot}962264$. When governed by this train, the polisher in relation to the fixed frame, rotates in the same direction as the arm with a velocity ratio of 0·172 for one revolution of the latter. The arm therefore revolves, in relation to it, with a velocity of 828 : 172. The result of these relative speeds is the production, by a point in the periphery of the polisher, of an epicycloid of five loops, or, more exactly, thirty-four loops for seven rotations of the polisher; for in this case the excentricity of the pin G becomes the

radius of the epicycle. Around this epicycloid of five loops the hypocyclic loops due to the excentricity of the spindle *k* are so distributed by a point in the periphery of the polisher that for one complete rotation of it 129 loops are produced, while its centre describes the hypocycloid of twenty-two loops, as in the former case; but these are repeated 5·814 times for one rotation of the periphery of the polisher. This is the only train I have yet used in polishing, and is that which has given the good results alluded to at the commencement of this paper.

The various figures described in the preceding paragraphs are very much modified by the cross-motion of the machine, and also by the rotation of the speculum; but it would occupy too much space to describe the multiplicity of changes which arise.

From the foregoing statements it will be seen, that small differences in the rotation-velocity of the polisher must have a material effect in altering the relative grinding action of points at various radial distances on its surface; and hence, to ensure the reproduction of a desired result, this velocity must be taken into account as well as any other adjustable ratio in the machine.

With the view of studying the effect of any given rotation-velocity of the polisher in relation to all or any of the movements of the machine, I fix an arm on the hexagonal pin *k*, so as to represent a radius of the polisher. On this arm slide three pen-holders, in either or all of which I can place cylindrical pens, which are charged with inks of different colours. On putting the machine or any part of it into motion, the pens trace out the curves due to all or any of the movements, and thus afford a ready means of ascertaining the distribution of grinding effect. As the result of several trials of this kind, I may state that so great a difference is produced in the various figures by comparatively small alterations in the adjustments, that I believe this mode of investigation will prove of the highest value to the telescope-maker, by enabling him to form a good conjecture beforehand of the results likely to arise from various settings of the machine.

I desire to avail myself of this opportunity to acknowledge my obligations to Mr. Lassell for his ready communication, from time to time, of his methods of manipulation. I have also to express my acknowledgments to Mr. James Nasmyth, for many most valuable hints in polishing and grinding, more particularly for imparting to me his plan of producing a beautifully uniform surface to specula, previous to the final pitch-polishing, by means of a grinder made up of squares of German blue-stone (hone), cemented with a composition of pitch and rosin on to a light frame of cast-iron. I attribute much of any success which I may have had to the adoption of this plan, or a slight modification of it. My alteration consists in substituting a slab of common slate for the bed of hones; the former having the advantage of always retaining its figure, which the hone-tool does not when laid on one side for any length of time. On a future occasion I may have something further to communicate respecting the employment of the slate-tool.

New Solution of Kepler's Problem. By Professor P. A. Hansen.

(*Extract from a Letter to the Astronomer Royal.*)

"It is well known how much labour has been bestowed by geometers on the solution of Kepler's problem; and what complicated results have been obtained for the coefficients in the expression for the equation of the centre. I have lately found a new solution of this problem, which differs strikingly from former solutions in this respect, that it leads to an unexpectedly simple law of coefficients. It is as follows:—

Let g be the mean anomaly.
φ the angle of eccentricity.
i any positive integer number.

$$\text{Put } \beta = \tan\frac{\varphi}{2}, \qquad \mu = i\cos^2\frac{\varphi}{2}$$

$$P_i = 1 + \mu + \frac{\mu^2}{2} + \frac{\mu^2}{2.3} + \&c. + \frac{\mu^i}{2.3\ldots i} \qquad Q_1 = 1 - \mu$$

$$P_{i+1} = P_i + \frac{\mu^i + 1}{2.3\ldots i+1} \qquad Q_2 = Q_1 + \frac{\mu^2}{2}$$

$$\&c. \qquad Q_3 = Q_2 - \frac{\mu^3}{2.3}$$

$$\&c.$$

Then the equation of the centre =

$$(1-\beta^2)\,\Sigma_1^\infty \frac{2}{i}\left\{P_i \cdot \beta^i + P_{i+1} \cdot Q_1 \cdot \beta^{i+2} + P_{i+2} \cdot Q_2 \cdot \beta^{i+4} \cdot \right.$$

$$\left. + \&c. \right\} \sin i\,g.$$

"The analysis which has led me to this, and the form of this result, are by no means peculiar to the equation of the centre only, but apply to all functions which it is necessary to develope in series proceeding by sin $i\,g$, or cos $i\,g$."

On the Brilliancy of the variable Star α Orionis. By Mr. Isaac Fletcher, Tarn Bank.

"In a communication read before the Royal Astronomical Society on January 10, 1840, Sir John Herschel points out and establishes by indubitable evidence the variability and periodic nature of the star *α Orionis*. In that paper, Sir John states as the result of his numerous observations and comparisons, that the brilliancy of this star, when at its maximum, is superior to that of

Rigel, but most decidedly inferior to that of *Capella*, which of late years has undoubtedly been the brightest star in the northern hemisphere.

"On the evening of the 5th December, 1852, I was much struck with the extraordinary brightness of *α Orionis*; and notwithstanding its inferiority of altitude, on carefully comparing it with *Capella*, I had no hesitation in concluding *α Orionis* to be the brighter star. Nor was this to be attributed to any diminution of lustre in *Capella*; for in the earlier part of the evening, when *α Lyræ* and *Capella* were of nearly equal altitude, I compared them together, and *Capella* was unquestionably the more brilliant.

"As regards *α Orionis* and *Capella*, on the night in question, I appealed to the judgment of two friends, both of whom deliberately and repeatedly compared the stars together, and assigned the superiority to *α Orionis*. On the 6th December I made another comparison; and although I thought the difference in brightness was less obvious than on the previous evening, still I did not hesitate to assign the superiority to *α Orionis*; such was also the judgment of another person to whom I referred the matter.

"Next evening I again compared the stars, and although the neighbourhood of *Orion* was occasionally obscured by fog, I concluded *α Orionis* to be fully equal, if not superior, to *Capella*. The following evening the sky was too hazy for accurate comparison.

"I, however, consider this fact to be clearly established; viz. that on or about the 5th of December, 1852, *α Orionis* was at or near its maximum brilliancy, and that at that time *it was the brightest star in the northern hemisphere.*

"I hope the circumstance I have above recorded will induce those observers who are watching the phenomena of the variable stars, to pay particular attention to this object.

"*December 9th*, 1852."

Mr. W. Rothwell exhibited a small transit instrument on a peculiar construction.

The telescope, which is only 14 inches long, is fixed on a brass pillar, so as to look downwards at about an angle of 45°; it thus somewhat resembles in appearance a solar microscope as usually mounted. The brass pillar is attached to a horizontal plate, which has the necessary adjustments for setting it level, and also for giving it a small motion in azimuth. In the line of sight of the telescope, and pretty close to the object-glass, there is a horizontal axis at right angles to the telescope. This axis rests on Ys as an ordinary transit; it has the same adjustments and riding level. In the centre of the axis is placed a mirror, like the index-glass of a sextant, into which the telescope looks for all its objects. The instrument being placed approximately in the meridian, and the

horizontal plate and the axis being carefully levelled, the axis is turned round until the wires of the telescope, which are illuminated by a side-lamp, are seen by reflexion from the mirror. The wires seen thus reflected are made to coincide with the wires seen *directly*, by the screws at the eye-end, in the usual manner. The axis is then reversed, and the reflected and direct wires again made to coincide,—half by moving the collimation adjusting-screws, and half by the two screws at the back of the mirror. When the axis is returned to its original position, if the direct and reflected wires coincide, the reflecting surface will be perpendicular to the optical axis and parallel with the horizontal axis.

At one end of the horizontal axis there is an altitude-circle, which is fitted exactly as in the usual portable transit; the vernier shows the angle through which the mirror is turned. The mirror must be turned round till a known star is found and bisected by the horizontal wire, when a comparison of the vernier reading with the known polar distance of the star will enable the observer to set the vernier to the exact reading, north polar distance, or altitude, or zenith distance, which he requires.

The whole instrument must be set in the meridian by making the stars near the south horizon give the same clock-error as the stars near the zenith. Mr. Rothwell recommends this form of transit, not account of its superior accuracy, but superior convenience, and considers it as sufficient for ordinary purposes. The stand would probably be less liable to change if the mounting were *cut out of stone*. With nice workmanship, an excellent mirror, and great care, considerable accuracy might no doubt be attained. A meridian-mark would be a desirable check for rating time-keepers, if any well-defined object could be found or placed in the meridian.

Mr. Norman Pogson, who is now attached to the Radcliffe Observatory, writes, that after his regular work for the evening is over (transit-observing), he employs the equatoreal, on fine nights, in searching for new planets, using Mr. Bishop's and the Berlin charts.

Mr. Pogson uses two diaphragms. No. 1 has a vertical bar, which passes through the field in the direction of a meridian, straight on one side, and toothed on the other. The transits are observed at the straight edge, and the differences of polar distances noted approximately at the toothed side: the teeth are about 2′ apart. No. 2 is adapted for observing faint objects. This consists of two bars arranged as a V; one of these is set in the meridian, and on this the transits are taken; the time between the two bars gives, by a slight computation, the distance of each star from the angular point. The angle may be varied to suit specific objects, as was done by Lacaille, but for ordinary purposes 45° is a convenient opening.

The telescope is also furnished with a good ring micrometer,

with which Mr. Pogson made the observations of the small planets *Fortuna* and *Melpomene*, *Monthly Notices*, vol. xii. pp. 214, 217. The reductions are rather tedious, but when the difference of declination is less than 4′ or more than 6′, Mr. Pogson considers the results may be relied on. The different measures of the diameter of the ring agree very well.

Capt. Shea has presented some suggestions *on the cause of the large quantity of rain* which has recently fallen. His reasoning does not assign the phenomenon to celestial agency, but refers to the condition of the north polar region of the earth; the notice belongs, therefore, to meteorology rather than to astronomy.

In vol. 36 of the *Astronomische Nachrichten*, No. 844, p. 67, M. Luther, of the Bilk Observatory, has given an approximate ephemeris of *Parthenope* for the present opposition. The ephemeris is for Berlin mean midnight, and extends from Jan. 16 to Feb. 25, 1853.

Astronomical Observations made at the Radcliffe Observatory, Oxford, in the year 1850. By Manuel J. Johnson, M.A., Radcliffe Observer. Vol. XXI. royal 8vo. (pp. 300, Introduction pp. xliv). Oxford, 1852.

The explanations necessary for a due appreciation of this work are given in the Introduction. The observations were made chiefly in the meridian, but there are a few determinations with the heliometer.

A detailed description of the meridian instruments has been given in former volumes. The great mass of the objects observed are circumpolar stars. Subordinate to these are the observed places of a limited number of fundamental stars, for the determination of instrumental and clock errors. There are also a few observations of the sun, as well as of the moon and moon-culminating stars.

The results of the circumpolar observations are presented in a catalogue adapted to the mean equinox of 1850·0. The number of stars is 2009. This contribution will serve nearly to complete the important catalogue of circumpolar stars which for several years has occupied so much of Mr. Johnson's attention. The re-observation of Groombridge's Catalogue may be now considered to be finished, and Mr. Johnson has appended to this volume an "Index of Stars in Groombridge's Catalogue, observations of which will be found in the eleventh and preceding volumes of the Radcliffe Observations." Until the Radcliffe Catalogue shall be published in a collected shape, which may soon be expected, this index will enable any one to determine the place of a star with very little trouble.

The circumpolar catalogue is followed by a smaller catalogue

of fundamental and other stars, whose north polar distances exceed 50°. The number of these stars is eighty-five. After this catalogue there is inserted a comparison of the observed right ascensions and north polar distances of the sun and moon, with the corresponding results of theory, as deducible by interpolation from the *Nautical Almanac*.

The concluding part of the work is devoted to the observations with the heliometer. There is a detailed description of this instrument in the Introduction, accompanied with an engraving at the beginning of the volume. The observations, having been made for studying the peculiarities of the instrument rather than with reference to any particular astronomical purpose, are not published from any conviction of their intrinsic value. They consist of a few measures of double stars, and of the apparent diameters of *Mars* and *Jupiter*. The methods of measurement varied with the objects observed. For double stars, whose components were situate at a moderate distance, the objects were ranged in lines at equal intervals, the moving image being placed alternately east and west of the fixed image. If the distance was considerable, the components of the moving image were brought alternately midway on the line of distance between the components of the fixed image. Generally, however, in such cases the observations were made by contact. The number of double stars observed is twenty-six. There are, however, several measures of each star. The mean result of eight measures of *γ Virginis* assigns 2″·851 as the distance of the two components for the mean epoch, 1850·230. There are a few measures of *Mars*, which were executed in the beginning of January. They give 5″·901 and 6″·503 for the mean values of the polar and equatoreal diameters. From the measures of *Jupiter* Mr. Johnson has deduced an ellipticity amounting to $\frac{1}{15\cdot6}$.

Researches on the Apparent Diameter, the Ellipticity, and the Position of the Axis of Rotation, of the Planet Mars. By Dr. J. A. C. Oudemans, of Leyden. (*Ast. Nach.* No. 838.)

The observations which form the groundwork of these researches were made by Bessel at Königsberg, with the great heliometer. The interval embraced by them extends from 1830 to 1837. As they correspond to very different distances of *Mars* from the earth, they were deemed by M. Oudemans to constitute favourable materials for investigating the circumstances relating to the magnitude, figure, and rotation of the planet. No sensible indications of ellipticity were afforded by the observations. The mean value which they assigned to the semi-diameter corresponding to a distance equal to the mean distance of the earth from the sun, was 4″·664. The inclination of the axis of rotation to the ecliptic was found to be 61° 9′, and the longitude of the ascending node of the planet's equator to be 349° 1′.

Briefwechsel zwischen W. Olbers und F. W. Bessel, herausgegeben von Adolph Erman, in zwei Bänden. 8vo. Leipzig, Avenarius and Mendelsson, 1852.

The first volume contains a short preface by the Editor, v.–viii.; an autobiography by Bessel, up to the age of twenty-five, ix.–xxxvi.; and a catalogue of the letters with their dates, xxxvii.–xxxix. The correspondence, which commences with a letter from Olbers to Bessel, dated August 12, 1804, fills 448 pages of the first volume.

Volume II. has a catalogue of the letters with their dates, v.–vi., and then the correspondence in 442 pages, ending with a letter from Bessel to Olbers, 28th July, 1839.

The work altogether contains 279 letters by Olbers, and 318 by Bessel, almost all relating to astronomical subjects. To those who are at all acquainted with the writers, it is needless to say that this work *must be read* by every one who wishes to acquaint himself with the progress of astronomy in the present century.

The Achromatic Telescope and its various Mountings, especially the Equatoreal, &c. By William Simms, F.R.S. F.R.A.S. &c. 8vo. 74 pp. London, 1852. Troughton and Simms, 138 Fleet Street.

In this brief work Mr. Simms has explained the principles on which the achromatic telescope depends, and has described and figured the parts of which it is composed. He has given drawings of most of the equatorial mountings now in use, with their appendages, and sketches of a suitable observatory. The volume is adapted to the use of amateur astronomers, and in itself professes nothing more; but there are references to other works of a more extended scientific character.

Recherches sur le prochain Retour de la Comète de D'Arrest. Par M. Villarceau.

(*Comptes Rendus, Dec.* 6, 1852.)

Two sets of elements are assigned, the results in each case being affected by an indeterminate quantity. Supposing this quantity equal to zero, the next passage of the perihelion is fixed for 1857, Dec. 2·368, or 1857, Nov. 28·742, according as the one or the other set of elements is adopted.

Notice sur quelques Recherches récentes Astronomiques et Physiques relative aux Apparences que présente le Corps du Soleil. Rédigée par M. le Prof. Alfred Gautier. Geneva, 1852.

In this *brochure* M. Gautier has collected a variety of interesting facts relative to the solar spots and other kindred subjects, tending to throw light upon the physical constitution of the sun.

Troisième Mémoire sur les Etoiles Doubles. Par M. Villarceau.

According to this method the five coefficients of the apparent orbit are deduced from the *apparent* co-ordinates. The mean motion and the longitude of the epoch are then obtained by means of the time and the law of the areas.

Théorie Analytique de l'Inégalité de Lumière des Etoiles Doubles. Par M. Villarceau.

In his memoir upon double stars (*Connaissance des Temps*, 1830), Savary first drew attention to the inequality depending on the difference of the times which light occupies in passing from the two components of a double star to the earth, and he suggested the possibility of hence deducing an inferior limit of the parallax of the star. M. Villarceau having undertaken an analytical investigation of the subject, obtained four inequalities depending upon this principle. The first of these depends simply on the distance of the star from the sun; the second, on the circumstance of the star's approach to, or recess from, the sun; the third, on the proper motion of the star; and the fourth, on the relative masses of the two components. The three inequalities first mentioned are confounded with the elliptic motion of the star. The fourth, which depends on the relation between the masses of the stars, had not been hitherto recognised by any one. If this relation was known, the parallax which is involved with it in the equations of condition might hence be deduced. Since, however, this element is totally unknown, it is impossible to establish even an inferior limit of the parallax by means of such researches.

Note de M. Faye, sur une nouvelle Détermination de la Parallaxe de l'Etoile d'Argelander (1830, *Groombridge*). Par M. Wichmann.

(*Comptes Rendus, Dec.* 13, 1852.)

It is well known that M. Faye, by comparing the right ascension of this star with that of another small star situate nearly on the same parallel (whose parallax was supposed to be insensible), found its parallax to amount to 1″·08. Subsequently, however, M. Peters, by observations of declination, obtained a parallax of only 0″·28. M. Otto Struve, by comparing the observed declinations of the star at different seasons of the year with the corresponding declinations of two other small stars in its vicinity, found the parallax to be 0″·03; and M. Wichmann, from observations made at Königsberg with the great heliometer, deduced a parallax amounting only to 0″·18.

The observations with the Königsberg heliometer were made by comparing the star of Argelander (A) with two other small stars, a, a', whose parallaxes were supposed to be insensible, and the

parallax of the star A was determined by a discussion of the *differences* of its distances from the two stars of comparison. The *sum* of the distances, however, which ought to have been constant, exhibited an annual variation which could not be accounted for by any recognised principle. The hypothesis of this variation being attributable to the influence of temperature upon the different parts of the instrument leads to improbable conclusions. If, however, *one of the stars of comparison* be supposed to have a sensible parallax, the periodic variation disappears, and the observations are represented with considerable accuracy. M. Wichmann has recently executed a new series of observations with the heliometer, and by a discussion of them founded upon this hypothesis he has found the parallax of A to be 0″·68, and the parallax of a to be 1″·15. The former observations, treated in a similar manner, assign 0″·75 and 1″·19 as the parallaxes of the two stars. M. Wichmann's memoir will be found in Nos. 841–844 of the *Astronomische Nachrichten.*

Note on the Mean Density of the Superficial Crust of the Earth. By M. Plana. (*Ast. Nach.* No. 828.)

The researches of geometers have established, beyond doubt, that the density of the earth increases towards the centre. Assuming the densities of the successive strata to increase in arithmetical progression, Laplace has investigated the constant amount of increase for each successive stratum, and has hence deduced the mean density of the terrestrial spheroid (*Méc. Cél.* tome v. liv. xi.). In his researches on this subject, he supposes the density of the superficial stratum (ϱ) to be three times the density of the sea considered equal to unity. He remarks, that this assumption agrees very nearly with the density of granite. His expression for the density of any stratum is,

$$\varrho = (\varrho)\,(1 + e - e\,a),$$

in which a denotes the radius of the stratum (the mean radius of the superficial stratum being supposed equal to unity), and e the constant quantity by which the depth of each successive stratum, $1 - a$ is to be multiplied conformably to the assumed law of density. Admitting the ellipticity of the earth to be equal to 0·00326, Laplace found the value of e to be 2·349, and hence determined the mean density to be 4·764. This value differs considerably from the results which Reich and Baily have deduced from their experiments with the balance of torsion, the former having obtained 5·44, and the latter 5·6604, for the mean density of the terrestrial spheroid, the density of pure water being supposed equal to unity.

The remarks of Humboldt on the density of the superficial stratum of the earth, contained in the first volume of his *Kosmos*, would seem to imply that the value of this element assumed by Laplace is erroneous. He states, that from the nature of the rocks which constitute the superficial strata of the solid parts of the globe, the

density of continents is hardly 2·7, and he hence infers, that the mean density of continents and seas taken together does not amount to 1·6. The researches of Plana, contained in the note above referred to, serve to confirm this conclusion. Supposing the ellipticity of the earth to be represented by 0·00326 (1 — 0·008479), he has found that the mean density 5·44, and the initial density 1·6, may be satisfactorily accounted for. The ellipticity derived either from actual measurement or from researches on the lunar theory cannot be regarded as sufficiently trustworthy to render the value here assumed inadmissible. On the other hand, if the ellipticity be supposed equal to 0·00326, the mean density deducible is 4·76; a result which is incompatible with the precision of the experiments made for the purpose of determining this element.

Metis.— Ephemeris of this planet for the opposition of 1853, extending from September 17 to October 31, will be found in the *Ast. Nach.* No. 845.

Lutetia.— For elements and ephemeris, from January 8 to February 25, by M. Bruhns, see *Ast. Nach.* No. 846.

ERRATA.

Vol. XI., Index, page 250, 12 lines from the bottom, read as follows :—

Dawes, Rev. W. R., discovery of the inner dark ring of *Saturn*, on Nov. 29, 1850..... p. 22 *et seq.*

————————, correction of errors in printing the foregoing accounts... 49

Lassell, Mr., observation of the inner or dark ring of *Saturn*, Dec. 3, 1850... 21

Page 254, line 16,—

Saturn, inner dark ring of, discovery of, by Mr. Dawes, on Nov. 29, 1850.. 22, 49

——–, ————————, ————————, by Mr. Lassell, Dec. 3, 1850 21

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIII. *January* 14, 1853. No. 3.

J. C. ADAMS, Esq., President, in the chair.

Dr. Dick, Broughty Ferry, Dundee.
Capt. Ommanney, R.N.; and
G. V. Vernon, Esq., Manchester,

were balloted for and duly elected Fellows of the Society.

An Account of certain Experiments towards increasing facility and certainty in the Use of Mercury in Observations by Reflexion and for the Adjustments of Astronomical Instruments. By the Rev. C. Pritchard.

1. The difficulties and annoyances attending the use of mercury for instrumental adjustments are too well known by practical astronomers to require any description. In the present communication I propose to lay before the Society an account of certain experiments, which appear to disclose the source of these annoyances, and then to explain a method or methods by which I venture to believe they may be fully and with certainty and facility overcome.

2. My first attempt to modify or prevent the well-known tremors to which mercury is liable, consisted in placing a rim of amalgamated copper round the sides of the trough containing the mercury, and in roughening the bottom. I found this process diminished by half the quantity of mercury necessary to cover the bottom, but the tremors still continued, while the surface became dirtier than before.

3. The next expedient resorted to was the amalgamation of the *bottom* of the vessel, as well as that of the sides; thereby I found the quantity of mercury necessary to cover the bottom was about $\frac{1}{12}$th or $\frac{1}{15}$th of that required when the copper vessel was wholly unamalgamated. The surface of the mercury was also found to be exquisitely quiescent; the image of the wires in the telescope was decided and motionless, even when the meridian-shutters of the observatory were open, and the wind was blowing with sufficient force to extinguish the flame of gas: still the reflecting surface of the mercury was far from perfectly bright and clean. Attempts to remove the offending matter, either by skimming the surface or by

filtering the mercury through a cone of paper, failed to give a surface satisfactorily free from blemish.

4. Upon examining the mercury in the amalgamated copper vessel, after it had remained there untouched for twenty-four hours, and after gently inclining the vessel, I observed a phenomenon wholly unexpected. The entire surface was found to be covered with a somewhat tough film or pellicle, apparently consisting of long acicular crystals, starting, as it were, from the sides of the copper vessel, and chiefly from the lower parts of it, where the excess of mercury seemed to lift it up. If the vessel were carefully restored to its original horizontal position, the pellicle ceased to be visible: a small amount of violence fractured it, and then the line of fracture presented a permanent silvery line, reflecting but little light; upon further agitation the whole film broke up into minute particles, causing the surface of the mercury to assume somewhat of the appearance of frosted silver.

5. Upon pouring this mercury, thus contaminated, out of the shallow amalgamated vessel containing it, I found this film, or the greater portion of it, adhered so strongly to the bottom, that for a long time all my efforts to detach it were ineffectual. Wiping or gently scraping only served to divide it into smaller portions, and so, by floating on the top of any fresh mercury subsequently poured into the vessel, continued to injure the reflecting surface.

6. At length I devised a means of detaching this obnoxious film, by means of the percussion of a thin stream of mercury as it filtered through a cone of paper upon the bottom of the copper vessel held in a nearly vertical position. The film was thus gradually washed away and removed, and a small quantity of mercury, which was then suffered to remain in the vessel, presented the appearance of beautifully-faultless and highly-polished plate-glass. When this surface was placed under the telescope the reflexion of the wires was tremorless, black, and decided; so much so, that I was at first unable to distinguish the transit or the horizontal wires from their image.

7. This condition of the surface seemed to remain nearly unimpaired for five or six weeks, provided the vessel were permitted to remain undisturbed. Still, upon a closer examination, the existence of the pellicle or film was easily detected, and upon a slight agitation it broke up into the same small silvery portions I had before observed. Here, then, arose a formidable difficulty to the application of mercury thus treated to the observation of stars by reflexion, inasmuch as such observations require the repeated shifting of the reflecting surface.

8. In order to remove this obstacle, I tried the effect of amalgamation without the use of acid, conceiving that by some possibility there had been a formation of nitrate of mercury, inasmuch as the film under the microscope presented much of the appearance of this substance. But the film still made its appearance.

9. I then tried the effect of a vessel made of silver-electroplated copper. The adhesion of the mercury was perfect and in-

stantaneous, and the surface at first beautifully white, and perfect, and quiescent; but after a few hours the film, or a similar film, made its appearance just as before.

10. A copper vessel was then silvered by means of cyanide of potassium and oxide of silver: the surface of the mercury then became contaminated with a yellow film; but upon removing this by cyanide of potassium the reflecting surface was as perfect as in the former instances. Yet, after a few hours, the obnoxious film appeared as before.

11. Mercury placed in a tinned vessel presented a surface worse than any other observed.

12. It now became necessary to discover, if possible, what the pellicle was, and what was its origin. Hydrochloric acid was found to have no effect upon it; the film, therefore, did not consist of the oxide of a metal. Nitric acid entirely dissolved it, and upon applying the usual tests, the film appeared to consist entirely of metallic copper and metallic mercury, or of metallic silver and mercury, according as the vessel was either copper or silver. The mercury used was originally pure, but after standing in the shallow amalgamated vessels it was found to contain either silver or copper, but these metals only to a very slight extent, and in quantity by no means approaching the proportions in which they existed in the films. The source of these films now, therefore, appeared to be evidently the gradual floating up of the amalgams formed at the bottom of the vessels through the supernatant mercury. The existence, therefore, of these films seems to be an inherent and necessary consequence of amalgamation.

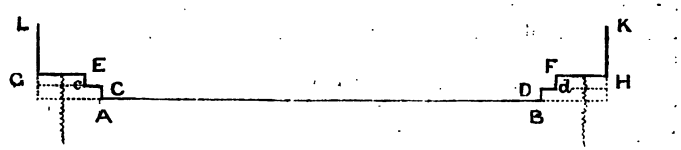
13. In this state of things I resolved to try the effect of a vessel made of platina. In order to effect the amalgamation of this metal, I in the first instance, adopted the very ingenious process pointed out by Mr. Grove, in an admirable paper printed in the *Philosophical Magazine for August* 1839, viz. that of making the platina dish, while in contact with mercury, the negative electrode in a voltaic circuit. Subsequently, however, I found that, provided the surface of the platina had been previously acted upon by nitro-muriatic acid, I could amalgamate the metal as perfectly and easily as I could copper, and that by simple friction only, without the intervention of the voltaic circuit. I had entertained the hope that the film of platina amalgam would now, on account of its density, remain at the bottom of the vessel, but it made its appearance at the surface of the mercury just as readily as in the case of a copper or silvered vessel, and with this additional disadvantage, that the film when broken up by agitation reflected less light than in the cases of copper or silver.

14. Thus the hopes I had entertained from the use of platina were not realized, but the object of these experiments had been so far attained that I could at any time procure a perfectly bright and comparatively tremorless surface of mercury, by means of the amalgamations and mercurial washing or percussion already described: this surface would remain effective for several weeks if undisturbed,

but did not, after a few hours, admit of unlimited removal from place to place.

15. Upon detailing these experiments to that very accurate philosopher, Professor Miller of Cambridge, he informed me that a paper upon the subject of the artificial horizon had appeared in Poggendorf's *Annalen*. On referring to p. 136 of the 79th volume, I found that M. Oertling, of Berlin, had there proposed an artificial horizon, constructed on the following principles:—1, a very shallow copper vessel was to be made, of a depth not exceeding $\frac{1}{120}$th of an inch; 2, this vessel was to be amalgamated by means of mercury and hydrochloric acid; 3, this exceedingly shallow vessel was to be placed upon a stand, supported by three adjusting foot-screws; 4, mercury was to be amalgamated to a very great extent with tin; 5, this amalgam of mercury and tin was to be poured into the shallow amalgamated copper vessel, and then the excess of mercury carefully skimmed off, leaving thereby a bright and tremorless surface. This surface, however, M. Oertling observes, is liable upon agitation, or from the access of wind, to become exceedingly foul. Of these five conditions, omitting or modifying four, and retaining only the fifth, viz. that of skimming off the surface, I now propose a construction of vessel, which I venture to regard as fulfilling all the requisites sought for in a perfect, and permanent, and movable reflecting surface. The surface itself is very easily obtained or renewed, and appears to be as applicable for the reflexion of stars as it is for the reflexion of the wires of a telescope. No adjusting-screws are necessary, no admixture of tin, no acid, nor any vessel of inconvenient shallowness, but such an one as may without difficulty be conveyed from place to place. The construction which I propose is as follows, admitting, however, such modifications as circumstances may require.

16. Upon a board of mahogany, or other wood, about $1\frac{1}{2}$ inch thick, is to be placed a perfectly flat rectangular plate of copper. On this copper is to be placed a flat rectangular rim, about $\frac{1}{20}$th or $\frac{1}{30}$th of an inch thick, and $\frac{1}{2}$ an inch wide. Thereby is formed a shallow rectangular vessel of copper, $\frac{1}{20}$th or $\frac{1}{30}$th of an inch deep. On three sides of this rim is to be placed a rim of iron or wood, $\frac{3}{8}$ths of an inch wide, and provided with a vertical flange $\frac{3}{8}$ths of an inch high; these three pieces of metal are to be screwed closely upon the mahogany. The section of the vessel thus formed is represented by the figure

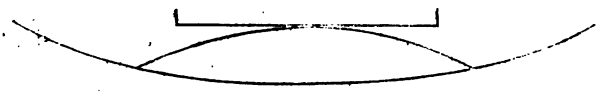


Where A B C D represents a section of the copper vessel, the depth A C = $\frac{1}{20}$th inch.

G E L is the iron or wooden angle-iron, forming a comparatively deep edge for the whole vessel.

The bottom A B, and the copper edges A C, D B, are to be carefully amalgamated by the friction of mercury with water and rotten stone, or pumice-stone, and then carefully washed with abundance of water, and dried by means of a piece of very clean linen. Pure mercury, free from lead or tin, is to be poured upon the bottom A B, and above the edges A C, B D. A flat piece of copper, with its edge amalgamated and cut so as to fit exactly the section G E C A D B F H, is to be drawn somewhat rapidly from the one end of the vessel to the other end, where there is no flange, so as to remove all the mercury, together with its floating impurities lying above the horizontal line C D, bodily into an iron box, placed in a suitable hole cut in the mahogany just under the extremity of the copper edge of the vessel. The whole is then to be tilted, until so much additional mercury has run out of the shallow vessel into the iron box as shall leave a depth of from $\frac{1}{80}$th to $\frac{1}{100}$th of an inch. The surface will be found to be perfectly quiescent, and free from all blemish, and may be carried with facility and without risk to any suitable position, and there placed upon a bed of sand. The sand I regard as very important, because it will at once facilitate the placing of the vessel in a horizontal position, and will also serve to destroy the propagation of tremors. It is evident, also, that the surface can, by means of mercury in the iron box, be renewed with the greatest facility in a very small space of time.

17. For ordinary purposes, it will be sufficient to form the vessel solely of flat copper turned up at the edges to the extent of about $\frac{1}{8}$th of an inch. This vessel is to be amalgamated according to the method directed, and then placed on the top of any vessel, such as a common saucer inverted in a large plate, as here represented :



Mercury can then be poured into the shallow amalgamated vessel, filling it above the brim; the excess of mercury, together with its floating impurities, can then be skimmed bodily into the plate, and any additional quantity afterwards be tilted out until a sufficient quantity only remains in the vessel.

18. The quantity of mercury required for an amalgamated circular vessel, five inches in diameter, is under two ounces troy. If unamalgamated, the least possible amount is about twenty-one ounces.

19. The mercury after continued use will ultimately become very dirty and contaminated with copper, but I have found that the impurities may be wholly removed by the agitation of the impure mercury in a bottle containing a very little water. The impurities assume the form of a dark spongy mass, and while floating on the surface of the wet mercury can be with facility removed. The

mercury itself can then be dried with bibulous paper, and after being gently heated becomes pure and bright.

20. I will only add, that the modifications which these experiments suggest in the construction of the common portable artificial horizon are very obvious. With regard to the ingenious arrangements which, after the completion of these experiments, Colonel Sabine informed me had been adopted some years ago by M. Schumacher for the artificial horizon, I found the form of the vessel, viz. the segment of a sphere, peculiarly objectionable, on account of the depth and the quantity of mercury employed: moreover, there is no possibility of easily cleaning the surface, or of removing the obnoxious film of amalgam so often alluded to.

On the Determination of the Orbit which a Satellite describes around its Planet. By M. Sawitsch, Professor of Astronomy in the University of St. Petersburg.

When there exist a great number of observations of a satellite, we may employ various combinations for finding with facility the elements of the orbit which the revolving body describes around its primary. It appears, however, that no method has yet been assigned for determining these elements from the smallest possible number of observations. The general theory which we owe to the illustrious Gauss, relative to the calculation of planetary orbits, undoubtedly applies also to the problem under consideration. This theory first leads to an approximate solution; after which it puts us in the way of obtaining such a degree of accuracy as the excellence of the observations demands. The provisional results, however, necessarily imply that the intervals between the times of observation are small, relatively to the complete revolution of the celestial body in its orbit. Such a supposition cannot be admitted when the question refers to a satellite, for in that case a single day may form a considerable part of the entire period of revolution. It is proposed, according to the following method, to determine the elements from three observations, the interval between the first and third being supposed not to exceed a few days.

Let t, t', t'', denote the times of the first, second, and third observations; Δ, Δ', Δ'', the observed distances of the satellite from the planet; and Q, Q', Q'', the angles of position referred to the circle of the geocentric latitude of the planet. Moreover, let l, l', l'', b, b', b'', denote the geocentric longitudes and latitudes of the planet: and R, R', R'', its corresponding linear distances from the earth.

All these quantities are supposed to be given either by observation or calculation; but the following are to be determined, viz.:—

R $(1+\delta)$, R' $(1+\delta')$, R'' $(1+\delta'')$, the linear distances of the satellite from the earth, corresponding to the times t, t', t''.

τ, the period of the revolution of the satellite.

ϱ, the angle under which the radius of the orbit of the satellite would appear at the mean distance of the planet from the sun.

i, the inclination of the orbit of the satellite to a plane passing through the centre of the planet parallel to the ecliptic.

ω, the longitude of the ascending node of the orbit measured upon the same plane.

ε, the longitude of the epoch.

Let the motion of the satellite, as well as that of its primary, be referred to a system of rectangular co-ordinates, the origin of which is supposed to be at the centre of the earth. Let us suppose that the axes of X and Y are in the plane of the ecliptic; also let the positive axis of X be directed to the longitude l', the positive axis of Y to the longitude $90° + l'$, and the positive axis of Z to the north pole of the ecliptic.

Let X, Y, Z, X′, Y′, Z′, X″, Y″, Z″, be the co-ordinates of the planet, and $X + x$, &c., $X' + x'$, &c., $Z'' + z''$, &c., those of the satellite corresponding to the times t, t', t''. Let dl, dl', dl'', represent the differences of geocentric longitude of the planet and satellite, and db, db', db'', the differences of latitude. Hence we have

$$X = R\cos b\cos(l - l'), \qquad X + x = R(1+\delta)\cos(b + db)\cos(l - l' + dl)$$
$$Y = R\cos b\sin(l - l'), \qquad Y + y = R(1+\delta)\cos(b + db)\sin(l - l' + dl)$$
$$Z = R\,.\sin b, \qquad Z + z = R(1+\delta)\sin(b + db).$$

Similar expressions may be obtained for the other co-ordinates of the planet and satellite.
We have also manifestly

$$dl = \frac{\Delta\,.\sin Q}{\cos b}; \qquad dl' = \frac{\Delta'\,.\sin Q'}{\cos b'}; \qquad dl'' = \frac{\Delta''\,.\sin Q''}{\cos b''};$$
$$db = \Delta\cos Q; \qquad db' = \Delta'\cos Q'; \qquad db'' = \Delta''\cos Q''.$$

Let us now transport the system of co-ordinates parallel to itself from the centre of the earth to the centre of the planet. The co-ordinates of the satellite relative to the centre of the planet will then be represented by $x, y, z, x', y', z', x'', y'', z''$. Remarking that $R\delta$, $R'\delta'$, $R''\delta''$, $\sin dl$, $\sin db$, $\sin dl'$, $\sin db'$, $\sin dl''$, $\sin db''$ may be considered as small quantities of the first order, the squares and products of which may be neglected; and bearing in mind further that $l - l'$, $l'' - l'$, do not amount, during an interval of several days, to more than a few minutes of space, we have without sensible error,

$$\left.\begin{aligned} x &= R\,.\,\delta\cos b - R\,.\sin b\,.\cos Q\,.\,\Delta\,.\sin 1'' \\ y &= R\,.\sin Q\,.\,\Delta\,.\sin 1'' \\ z &= R\,.\,\delta\sin b + R\cos b\,.\cos Q\,.\,\Delta\,.\sin 1'' \end{aligned}\right\} \text{(A)}$$

Similar expressions may be obtained for $x', y', z', x'', y'', z''$.

Let n denote the area of the triangle formed by the radii vectores of the satellite corresponding to the second and third observations, with the cord which unites their extremities. Let n', n'' represent the areas of the similar triangles corresponding to the first and third, and to the second and first observations. Since the satellite revolves in a plane passing through the centre of the planet, which coincides with the origin of the co-ordinates x, y, z, the following equations are deducible:—

$$nx - n'x' + n''x'' = 0 \qquad (1)$$
$$ny - n'y' + n''y'' = 0 \qquad (2)$$
$$nz - n'z' + n''z'' = 0 \qquad (3)$$

Substituting in (2) the values of y, y', y'', as already determined, we have

$$\frac{n'}{n} = \frac{R \,.\, \Delta \,.\, \sin Q}{R' \,.\, \Delta' \,.\, \sin Q'} + \frac{n''}{n'} \cdot \frac{R'' \,.\, \Delta'' \,.\, \sin Q''}{R' \,.\, \Delta' \,.\, \sin Q'} \qquad (\alpha)$$

Multiplying (1) by sin b', and (3) by cos b', and subtracting the one result from the other, bearing in mind at the same time, that as the geocentric latitudes of the principal planets are generally small, their alterations, $b - b'$, $b'' - b'$, in the space of a few days are inconsiderable, we have

$$\frac{n'}{n} = \frac{R \,.\, \Delta \,.\, \cos Q}{R' \,.\, \Delta' \,.\, \cos Q'} + \frac{n''}{n} \cdot \frac{R'' \,.\, \Delta'' \,.\, \cos Q''}{R' \,.\, \Delta' \,.\, \cos Q'} \qquad (\beta)$$

If we suppose the satellite to revolve in a circular orbit whose radius is r, we may employ the equations (α) and (β) in finding τ the period of revolution.

Let v'', v', v, be the angles described by the radius of the satellite during the intervals of time $t' - t$, $t'' - t$, $t'' - t'$. We have then

$$n = \frac{1}{2} r^2 \sin v, \; n' = \frac{1}{2} r^2 \sin v', \; n'' = \frac{1}{2} r^2 \sin v''$$

$$\frac{n'}{n} = \frac{\sin v'}{\sin v}, \qquad \frac{n''}{n} = \frac{\sin v''}{\sin v}$$

$$v = \frac{360^\circ}{\tau}(t'' - t'), \; v' = \frac{360^\circ}{\tau}(t'' - t), \; v'' = \frac{360^\circ}{\tau}(t' - t)$$

It is clear, then, that if an approximate value of τ was known, we might easily find v, v', v'', and hence also the relations $\frac{n'}{n}$, $\frac{n''}{n}$. Now it is always possible to obtain such a value of τ by successive trials. Assuming $\tau = 3$ or 6 or 10, &c. days, we calculate the corresponding values of $\frac{\sin v'}{\sin v}$, $\frac{\sin v''}{\sin v}$, and then we shall see which of the hypotheses satisfies best the equality of $\frac{\sin v'}{\sin v}$ and $\frac{n'}{n}$ as determined by (α) or (β). The corresponding value of τ will express the approximate value of the period of revolution. In order to avoid useless calculation, it will be desirable to take into consideration

the three angles of position Q, Q′, Q″, which will serve to show if the interval of time $t'' - t$ between the first and third observations approaches nearly or not to the time of a complete revolution. Knowing τ approximately, and repeating the calculation for two hypotheses of that element, differing slightly from each other, we may obtain a more accurate result, and in this manner we may approach nearer and nearer to the true value. When the angles Q, Q′, Q″, are very small, or very nearly equal to 180°, it will be advantageous to employ equation (β) instead of equation (α).

The following example, based upon three of M. Otto Struve's observations of *Neptune's* satellite, will serve to illustrate the practical utility of the method :—

Date.	M. T. Pulkowa.	Angular Distances.	Angle of Position with respect to the Circle of Latitude.
1847. Sept. 25	$10^h\ 21^m = t$	$15''{\cdot}92 = \Delta$	$228°\ 45' = Q$
28	$12^h\ 9^m = t'$	$17''{\cdot}30 = \Delta'$	$57°\ 42' = Q'$
30	$9^h\ 36^m = t''$	$5''{\cdot}07 = \Delta''$	$164°\ 51' = Q''$

Hence

$$t' - t = 3^d{\cdot}075,\ t'' - t' = 1^d{\cdot}894,\ t'' - t = 4^d{\cdot}969.$$

Moreover, the ephemeris of *Neptune* gives

$$\log R = 1{\cdot}46450,\ \log R' = 1{\cdot}46505,\ \log R'' = 1{\cdot}46540$$

It will be seen at once that the time of a complete revolution exceeds five days, for in 4·969 days the angle of position (Q″) has not nearly resumed its original value (Q). We may then commence by supposing $\tau = 5{\cdot}5$ and $\tau = 6$. The former of these hypotheses gives for $\frac{\sin v'}{\sin v}$ a numerical value by far *too small* in comparison with its equivalent $\frac{n'}{n}$ calculated from the formula (α). Taking $\tau = 6$, we obtain for $\frac{\sin v'}{\sin v}$ a value *too great*. If we now assume $\tau = 5{\cdot}7$ and $\tau = 5{\cdot}8$ we obtain

$$\log\frac{n'}{n} - \log\frac{\sin v'}{\sin v} = +0{\cdot}00643\ (\tau = 5{\cdot}7)$$

$$\log\frac{n'}{n} - \log\frac{\sin v'}{\sin v} = -0{\cdot}02390\ (\tau = 5{\cdot}8)$$

Thus the one hypothesis is 0·00643 in *excess*, and the other, 0·02390 in *defect*, and hence by proportional parts we find $\tau = 5{\cdot}72$. Repeating the calculation with $\tau = 5{\cdot}72$, we shall find that this hypothesis is erroneous only to the extent of + 0·00113, and we obtain definitively $\tau = 5{\cdot}724$ days. By a discussion of the totality of eighteen observations made in the years 1847–49, M. A. Struve found $\tau = 5{\cdot}876$ days, a result which accords sufficiently well with that deduced by an approximative process from three isolated observations.

When once we know the value of r we may easily determine the radius of the orbit of the satellite.

Let r denote the linear value of this radius; A the semi-major axis of the orbit of the planet around which the satellite revolves; v'' the angle included between the two radii whose extremities are determined by the co-ordinates x, y, z, x', y', z'. We have then

$$\cos v'' = \frac{x x' + y y' + z z'}{r^2}$$

substituting for x, y, z, x', y', z' their values (A); bearing in mind also that $\varrho = \frac{r}{A \sin 1''}$ and that $r^2 = x^2 + y^2 + z^2 = x'^2 + y'^2 + z'^2$, we finally obtain

$$\varrho = \sqrt{\tfrac{1}{2} K^2 \pm \sqrt{\tfrac{1}{4} K^4 - M^2}}$$

in which

$$M^2 = \frac{\left(\frac{R}{A} \cdot \Delta\right)^2 \left(\frac{R'}{A} \cdot \Delta'\right) \cdot \sin^2 (Q' - Q)}{\sin^2 v''}$$

$$R^2 = \frac{\left(\frac{R}{A} \cdot \Delta\right)^2 + \left(\frac{R'}{A} \cdot \Delta'\right)^2 - 2 \left(\frac{R}{A} \cdot \Delta\right) \left(\frac{R'}{A} \cdot \Delta'\right) \cos (Q' - Q) \cos v''}{\sin^2 v''}$$

It is easy to select the proper sign of the quantity under the radical in the expression for ϱ, since the latter quantity is essentially greater than $\frac{R \cdot \Delta}{A}$, $\frac{R' \cdot \Delta'}{A}$, or $\frac{R'' \cdot \Delta''}{A}$. We may determine ϱ in a similar way by combining the third observation with the first or second. The most favourable combination is that in which $\sin v$ has the greatest value.

In the above example we have,

$$Q = 228^\circ 45' \qquad Q' = 57^\circ 42'$$

$$v'' = 193^\circ 22'; \qquad \frac{R}{A} \cdot \Delta = 15''{\cdot}445; \qquad \frac{R'}{A} \cdot \Delta' = 16''{\cdot}806$$

$$\tfrac{1}{2} K^2 = 206{\cdot}3; \qquad M^2 = 30624; \qquad \sqrt{\tfrac{1}{4} K^2 - M^2} = 109{\cdot}2$$

$$\varrho^2 = 315{\cdot}5; \qquad \varrho = 17''{\cdot}762$$

M. A. Struve has deduced $\varrho = 17''{\cdot}969$ from the totality of 18 observations.

We have now to calculate the planetocentric longitudes and latitudes of the satellite, $\lambda, \lambda', \lambda'', \theta, \theta', \theta''$, with respect to a plane passing through the centre of the planet parallel to the ecliptic. The expressions of the three planetocentric co-ordinates x, y, z, in which the direction of the axis of x is determined by the longitude l, lead to the following equations:—

$$\varrho \cdot \cos\theta \cdot \cos(\lambda - l) = \xi \cos b - \sin b \frac{R}{A} \cdot \Delta \cdot \cos Q \qquad (a)$$

$$\varrho \cdot \cos\theta \cdot \sin(\lambda - l) = \frac{R}{A} \cdot \Delta \cdot \sin Q \qquad (b)$$

$$\varrho \sin\theta = \xi \sin b + \cos b \cdot \frac{R}{A} \cdot \Delta \cdot \cos Q \qquad (c)$$

where $$\xi = \pm\sqrt{\varrho^2 - \left(\frac{R}{A} \cdot \Delta\right)^2}$$

Dividing (b) by (a) we find an expression for tan $(\lambda - l)$, and as l or the geocentric longitude of the planet for the time t is known, we thus obtain the value of λ. The equation (c) serves to calculate θ, the planetocentric latitude of the satellite.

If we suppose the direction of the axis of x to be determined by the longitude l', and then by the longitude l'', we shall obtain equations analogous to (a), (b), (c), which will serve to calculate λ', θ', and λ'', θ'', by means of the data relative to the second and third observations.

When we have determined λ, θ, λ', θ', the following formulæ will serve to calculate the inclination i, and the longitude Ω of the ascending node of the orbit of the satellite upon a plane passing through the centre of the planet parallel to the ecliptic.

$$\sin\left(\frac{\lambda' + \lambda}{2} - \Omega\right), \tan i = \tfrac{1}{2}(\tan\theta' + \tan\theta) \sec\tfrac{1}{2}(\lambda' - \lambda)$$

$$\cos\left(\frac{\lambda' + \lambda}{2} - \Omega\right), \tan i = \tfrac{1}{2}(\tan\theta' - \tan\theta) \operatorname{cosec}\tfrac{1}{2}(\lambda' - \lambda)$$

We might obtain i and Ω in a similar manner by combining λ'', θ'', with λ', θ', or with λ, θ.

In our example,

by combining λ', θ', with λ, θ, we have $\Omega = 264^\circ\ 46'$; $i = 37^\circ\ 35'$
— — λ'', θ'' — λ', θ' — $\Omega = 296\ 36$; $i = 30\ 28$

The difference of these two determinations, although sufficiently great, does not depend upon calculation. It arises from very small errors to which the best observations are liable, and which may exercise a very sensible influence according to the positions of the satellite with respect to the nodes at the times of observation. We may remark that the combination of the values λ, θ, λ', θ', is not favourable for determining Ω, since the satellite was in those positions nearly -90° and $+90^\circ$ from the node. The combination of the second observation with the third is the most favourable for this purpose. As regards i both combinations are almost equally good. We admit, then,

$$\Omega = 296^\circ,\ i = 34^\circ$$

With these results, the equations

$$\tan u = \tan(\lambda - \Omega) \sec i;\ \Lambda = u + \Omega$$

will give the arguments of latitude u, u', u'', and the planetocentric longitudes $\Lambda, \Lambda', \Lambda''$, of the satellite in its own orbit.

In our example, $\Lambda = 201^\circ\ 34'$, $\Lambda' = 34^\circ\ 0'$, $\Lambda'' = 148^\circ\ 16'$. Designating by μ the mean daily motion of the satellite, we have—

$$\mu = \frac{\Lambda'' - \Lambda}{t'' - t} = 61^\circ{\cdot}72 \text{ by the interval of nearly 5 days.}$$

$$\mu = \frac{\Lambda' - \Lambda}{t' - t} = 62{\cdot}64 \quad — \quad — \quad 3 \ —$$

$$\mu = \frac{\Lambda'' - \Lambda'}{t'' - t'} = 60{\cdot}35 \quad — \quad — \quad 2 \ —$$

Whence the probable value of the mean daily motion,

$$\mu = 61^\circ{\cdot}72,$$

and the period of revolution,

$$\tau = \frac{360^\circ}{\mu} = 5{\cdot}832 \text{ days} = 5^d\ 19^h\ 10^m.$$

With $\mu = 61^\circ{\cdot}72$ we find the planetocentric longitude of the satellite upon its orbit, in 1847, September 28, at $12^h\ 9^m$ mean time to be—

By the first observation and the interval of $3{\cdot}075^d$				$31^\circ\ 14'$
second	—	—	0	..... $34\ 0$
third	—	—	$1{\cdot}894$	..... $31\ 26$
			Mean value	..... $32\ 13$

The corresponding argument of latitude is $96^\circ\ 13'$. Subtracting the motion in 11·01 days, we have $136^\circ\ 45'$ for the argument of latitude on the 11th of September, at $11^h\ 55^m$ mean time. M. A. Struve determines the same argument, by means of eighteen observations, to be equal to $128^\circ\ 26'$. We are, therefore, in error only to the extent of $8^\circ\ 20'$.

As the inclination of the orbit of *Neptune* to the ecliptic is $= 1^\circ\ 47'$, and as the longitude of the ascending node of the orbit is $= 130^\circ\ 7'$, we hence deduce the longitude of the ascending node of the satellite upon the orbit of *Neptune* to be $= 296^\circ\ 37'$, and the inclination to be $= 35^\circ\ 44'$. We have now to explain the modes of verifying what has been already found, and to assign results of greater accuracy.

Since the trustworthiness of the preceding calculations depends upon the precise knowledge of τ the period of revolution, which has only been determined approximately in the first instance, it will be desirable to ascertain whether this value accords sufficiently well with the value $\tau = \frac{360^\circ}{\mu}$. The latter value ought to be in general the more accurate of the two, and if it differs considerably

from τ as determined by means of (α) or (β), it will be necessary to repeat the calculations.

Assuming $\mu = 61°{\cdot}72$, we have $\tau = 5{\cdot}832$ days. By the first calculations we obtained $\tau = 5{\cdot}724$. The difference does not amount to more than 0·108 days.

Let us now inquire into the degree of approximation of the angle ϱ as already determined. With $\mu = 61°{\cdot}72$ we have—

$$v'' = 189° 40'\,;\quad Q' - Q = 188° 58'\,;\quad \frac{R\,\Delta}{A} = 15''{\cdot}445\,;\quad \frac{R'\,\Delta'}{A} = 16''{\cdot}806\,;$$

whence $\varrho = 20''{\cdot}40$

$$v = 116° 54'\,;\quad Q'' - Q' = 107° 8'\,;\quad \frac{R''\,\Delta''}{A} = 4{\cdot}929\,;\quad \frac{R'\,\Delta'}{A} = 16''{\cdot}806\,;$$

whence $\varrho = 18{\cdot}27$

$$v' = 306° 42'\,;\quad Q'' - Q = 296° 54'\,;\quad \frac{R''\,\Delta'}{A} = 4{\cdot}929\,;\quad \frac{R\,\Delta}{A} = 15{\cdot}445\,;$$

whence $\varrho = 18{\cdot}02$.

As the errors Δ, Δ', Q, Q', produce upon the determination of ϱ an influence so much greater, as sin v'' is less, we may take—

$$\frac{20''{\cdot}40 \sin v'' + 18''{\cdot}27 \sin v + 18''{\cdot}02 \sin v'}{\sin v'' + \sin v + \sin v'} = 18''{\cdot}35,$$

for the probable value of ϱ, as deducible from three observations. A new calculation of Ω and i would diminish only in a small degree the uncertainty of these elements.

It remains for us to say a few words upon the mode of obtaining the elliptic elements of the satellite. Although four observations suffice *theoretically* for that purpose, still, considering the small value of the excentricity and the sensible influence of errors of observation, it will be impossible to attain the object by means of these data alone. Before undertaking the calculation of the elliptic elements it is desirable to ascertain the circular elements, and *that* to a certain degree of precision, so as to treat their corrections as small quantities of the same order of magnitude as the excentricities. It is necessary, besides, to have a long series of good observations, in order to deduce from them the elliptic elements of the orbit in a trustworthy manner.

The first thing to be done is to ascertain the degree of accuracy with which the observations are satisfied by the circular elements. For this purpose, it will be necessary to calculate with these elements the angle of position Q_0 and the apparent angular distance Δ_0 for the time of each observation. Representing by Δ, Q, the values of these quantities as deducible from the corresponding observations, we then form the differential quantities $\Delta - \Delta_0$, $Q - Q_0$, which will serve to exhibit the errors of the existing hypothesis respecting the orbit of the satellite. Now if the orbit be supposed to be in reality an ellipse, $\Delta - \Delta_0$ and $Q - Q_0$ may

generally be expressed in terms of the corrections of the circular elements and the excentricity and longitude of the perihelion. Let e be the excentricity and π the longitude of the peri-planet; then, putting $e \cos \pi = \psi$ and $e \sin \pi = \eta$, we may express $\Delta - \Delta_0$ and $Q - Q_0$ in terms of seven unknown small quantities, viz.: the corrections of the five elements already involved in the circular hypothesis and the quantities ψ and η. An equation of condition involving these quantities may be formed for every observation, whether of distance or position; and from the totality of such equations, the values of the unknown quantities may be deduced by the application of the method of least squares.

Extracts of a Letter from M. Otto Struve to the Astronomer Royal, dated $\frac{15}{3}$ January, 1853.

"As to what regards Villarceau's remarks on the differences in right ascension of feeble comets, as observed by different instruments and methods, I am quite of opinion that similar differences must exist, and that for different reasons, but principally produced by the different optical power of the telescopes. The nature of these differences is, perhaps, not yet sufficiently explained for all different methods of observation; but, generally speaking, the most powerful telescopes must furnish the most exact results; and for this reason, I think, Villarceau has done wrong to attribute a higher precision to the Königsberg observations than to those made with the Northumberland telescope, especially as Wichmann himself thinks the accuracy of his observations of D'Arrest's comet has seriously been injured by the faintness of the object, while Challis does not mention at all that these observations have given him particular trouble."

"Last month the University of Dorpat celebrated the fiftieth anniversary of its renewed existence. On this occasion I wrote a dissertation, entitled *Narratio de Parallaxi Stellæ α Lyræ*. In this tract I have given, besides some general remarks on the determination of parallaxes, the result of fifteen months' observation on the parallax of *α Lyræ*. The comparison star used by me is identical with that of my father; but the result does not so well agree with the former, as I thought it would do. My observations, which, as to accuracy, appear to be considerably superior to those of my father, make the parallax by the

$$\text{Angles of position} = +0''.145 \pm 0''.016$$
$$\text{By the distances} = +0''.140 \pm 0''.025$$

"In combining these results with the former values obtained by W. Struve and Peters, the parallax of *α Lyræ* is found in the mean $= +0''.1549 \pm 0''.0120$.

"I have indicated in the same tract several reasons, which will account in part for the larger parallax found by my father, but the

principal part of the difference must be attributed to an accumulation of accidental errors: since last summer I am engaged in a similar way with the parallax of 61 *Cygni*.

"The present winter is again very unfavourable for astronomical observations; since nearly six weeks we have not seen the sun. It is for this reason that I have not yet been able to publish my observations on Biela's comet, which require a new determination of the comparison star. The number of observations I have got of that object is very limited. They consist of five observations of the northern head, and three of the companion, made all in the interval between the 18th and 28th September, 1852; but I think they are all pretty exact, and will probably prove important, as it appears that this object has not been observed elsewhere, but by Prof. Secchi at Rome.* It was very interesting to see that the mutual action of the two heads still appeared to continue, notwithstanding their considerably increased relative distances. On the first day, Sept. 20, the southern head was larger than the other, and five days afterwards (Sept. 25) the northern head was brighter and considerably larger. It was obvious that the principal stream of light followed in both heads almost the direction of the line which joined the two heads.

"Mr. Lassell has sent me from Malta a circumstantial notice of his observations of *Saturn's* rings. Besides the transparency of the obscure ring, the most interesting observation appears to be the disappearance of the division in the obscure ring, which is confirmed by my own observations of the present winter. This division being easily seen last year, and with perfect surety, there is no doubt now, that the obscure ring undergoes rapid changes in very short intervals.

"It has given me great satisfaction to learn that the British Association has recommended to the Royal Society a new research on the irradiation of the Huyghenian telescope. The existing uncertainty in the amount of this quantity is, I think, the principal objection to be made against the results I have deduced. The result of the proposed research will, therefore, decide the question, which I could only exhibit in my memoir as the most probable result to be derived from the existing observations."

Extracts of a Letter from Sig. Secchi to Captain Manners, R.N. dated January 28, 1853.

"We have given instructions to Mr. Mertz to furnish us with a complete refractor, of 9 inches aperture and 14 feet focal length. The erection of the new observatory is going on well, and I hope before summer to have the principal part of the building itself quite

* At the time of writing this letter M. Struve was not aware that the comet had been observed at the Berlin Observatory (*Ast. Nach.* No. 832, p. 246), and also at Cambridge, by Professor Challis (*Ast. Nach.* No. 836, p. 325).—R. G.

ready. We shall have a revolving dome, 23 feet in diameter, for the equatoreal, and an elliptical room for the meridian circle, 20 feet long and 16 wide. In this a small transit instrument, 4 feet focal length, will be fixed as a collimator: and it will also have a strong cast-iron stand, to be used as a portable transit or prime vertical instrument. The Cauchoix refractor will be kept to make supplementary observations to the large equatoreal. A description of the new observatory will be published as soon as it will be in working order."

"Mr. Lassell has been kind enough to send me a very beautiful drawing of *Saturn*. Among the particulars which attracted his attention there is, as you know, the outline of the shadow, which is terminated by a prominent point at the place of the division of the rings; the appearance seemed to me a new proof that the rings are not plane; and in order to assure me of this fact experimentally, as far as was possible, I got made an artificial system like that of *Saturn*, having the surface of the ring slightly curved, as I imagine the real rings to be. The artificial rings have a curvature of about 1-20th of an inch, with a breadth of 3 inches. I exposed them to a very strong light of artificial lamps, and to the light of the sun itself, always taking care to have the incident rays inclined to the plane of the rings, according to their position in the heavens. Looking then through a telescope at this system, from an elevation like that of the earth, over the plane of the rings, I was struck with the perfect resemblance of appearances, as were several others, who witnessed the same. The curvature may be a good deal less than that used in my experiment, and still the phenomenon appears the same, as I deduced from another rough trial. I am, therefore, disposed to believe that the apparent curvature of the shadow of the ball over the ring, which turns the convexity towards the ball, is not a mere illusion of contrast of curvature but a real fact. I hope that, with the new instrument, something more satisfactory will be seen."

"The method of observing the sun with small diaphragms succeeds very well, and astronomical science will be very much indebted to Mr. Dawes for this simple contrivance. But to prevent the heating of the eye-piece, I have found that a diaphragm on single thick paper, covered with white lead, as used on visiting-cards, is perhaps the best. After some hours of observation the eye-piece is only slightly hot with our 6-inch object-glass refractor. Looking at the sun with this diaphragm on December 28, 1852, and January 1 of the present year, I observed some very interesting appearances in a very remarkable large spot. The most striking to me was that of a kind of very large ribbon, of almost oval shape, very like the edge of a lunar crater, but one part of which was projected like a bridge over a very black deep part of a nucleus. This appearance lasted only some hours, yet, during this interval of time, the outline of the crater and bridge was quite changed. I found some resemblance between these and the spots of the figures 6, 7, 10 of Sir John Herschel's observations at the Cape

of Good Hope. Another striking appearance was, that the edge of the inner penumbra was all like a saw, as if many streamlets of fluid matter were running towards the centre of the nucleus through several obstacles, or over a very rough bottom. The changes of this spot were very great, and I made several measures and drawings, but want of space prevents me telling you more. These appearances, however, seem to me very characteristic, and may throw great light on the physical constitution of the sun."

"The faculæ are, I think, fully proved to be prominences, by the fact that they are a good deal larger near the edge than at the centre of the disc. This I have observed several times during my experiments on solar heat. Some of these faculæ remained constantly visible during half a revolution of the sun, so that their identity was assured. I mention this merely as a confirmation of Mr. Dawes' discovery. I think the faculæ are brighter, only because their tops exceed the lower part of the solar atmosphere, which may absorb a good deal of light, since it absorbs at least nine-tenths of the total heat of the sun."

Accompanying the letter from which the foregoing passages have been extracted were a series of observations of *Massalia*, extending from November 18, 1852, to January 6, 1853, which were made by Signor Secchi at Rome, with the Cauchoix equatoreal. There were also two observations of Westphal's Comet, one made on the 5th, and the other on the 30th of December. The latter observation is interesting, as being considerably later than any hitherto published. The observations both of the planet and comet will, doubtless, appear in the *Astronomische Nachrichten*. In the meantime it may be useful to insert those relating to the comet. The following are the apparent positions:—

1852.	Rome M.T. h m s	App. R.A. m s	App. Decl. ′ ″
Dec. 5	7 41 58	*m* + 5 41·20	*m* + 13 36·3
5	7 55 5	*m* + 5 40·90	*m* + 13 37·8
30	7 39 32	*n* − 0 45·15	*n* − 10 55·2
30	7 45 57	*n* − 0 45·30	*n* − 10 52·8

The approximate places of the stars of comparison are—

	R.A. h m s	Decl. ° ′
m	13 52 (20)	+ 66 4
n	13 11 (50)	+ 69 10

Letter from Mr. Lassell to Mr. De La Rue.

"I observe that your attention has continued to be drawn to the figure of *Saturn*, and that you have made a number of measures of his diameters; and also that Mr. Dawes has made some, though not under favourable circumstances. My drawings of *Saturn* did not profess to give the ellipticity in conformity with any measures

whatever, and I gave that form to the ball which satisfied my eye best on a comparison with the telescopic image. I have during my sojourn here made several sets of measures of the equatoreal and polar diameter, and the result is that $e = 0{\cdot}078764$ or $\frac{1}{12{\cdot}697}$ this compression (*apparent* compression — no correction being applied for present inclination of the axis) does not differ greatly from your own measures given in p. 21 of the *Notice*. Curiosity has led me to refer to my copy of the drawing sent last to the Society, and I find the compression is there graphically given *less* than the measurements show, viz. 0·05485, or $\frac{1}{18{\cdot}23}$ so that the drawing does not differ *so much* from the circle as either your own or my measures warrant.

"I presume that the 'something' which you thought you had discovered, but did not reveal in your note of the 20th November, is some inclination of the obscure ring to the plane of the others, evinced by an apparent encroachment of the dark ring on the bright one, and a want of symmetry in its elliptic projection. I can only remark upon this, that I have not observed such appearances; and whether I might have been able in any degree to confirm your observation, if my attention had been especially directed to that point, I cannot now say. The weather has been lately unfavourable, even here, and *Saturn* is getting fast away now into the twilight.

"*Valetta, Feb. 7, 1853.*"

Letter from the Rev. W. R. Dawes to Mr. De La Rue.

"Since I met you at the January meeting of the Astronomical Society, I have been looking over some observations of the diameters of *Saturn*, which I obtained in 1848. They are but few — only two sets of each diameter; but the measures were taken under remarkably favourable circumstances. In my journal, I have noted that the planet was *exquisitely* defined, and all the measures excellent. As they were so few, and it was probable that the larger telescopes in some of the public and private observatories would be specially directed to the subject, I made no use of them, and was not aware of the ellipticity they indicated. On reducing them and calculating the result, I find from one of the sets, $e = \frac{1}{12{\cdot}163}$, and from the other, $e = \frac{1}{11{\cdot}813}$; the mean being $e = \frac{1}{11{\cdot}988}$, which is somewhat larger than I should have expected from my impression of the appearance of the planet at that time. Yet on drawing an ellipse of these proportions, it does not appear preposterous, as you will perceive in the diagram inclosed, along with one representing the ellipticity according to Mr. Main's observations. With respect to these, the Astronomer Royal has informed me that, at the time, such an ellipse was deemed *satis-*

factory to the eye when compared with the planet as seen in the telescope; so that I must withdraw my too hasty remark on that point, in a recent letter to yourself, which I perceive is inserted in the number of the *Monthly Notices* for November 1852. If I had had the opportunity of revising it, I should probably have modified the substance of the remark, and certainly the manner of it.*

"In the *Monthly Notices*, vol. x. p. 7, are some measures of the diameters of *Saturn* (10 of each) by the Rev. R. A. Thompson, with the Durham Fraunhofer equatoreal, which give $e = \frac{1}{11{\cdot}233}$. I remember being struck with the extraordinary compression shown in the diagrams of *Saturn* by Julius Schmidt, of the Bonn Observatory, contained in No. 650 of the *Astronomische Nachrichten*. On turning to them, I find they are evidently drawn to a scale, representing, probably, the results of observations; but *what* observations I do not know, as none are mentioned, I believe, in Schmidt's paper. The proportions of the diagrams give $e = \frac{1}{9{\cdot}667}$. It is to be regretted that the ellipticity assumed in the *Nautical Almanac* is not stated in the preface. Computed from the data for 1853, Sept. 1, it seems to be $e = \frac{1}{13{\cdot}035}$; but I am not aware from what observations this is deduced: and the result can be only approximate.

"*Wateringbury, Feb.* 12, 1853."

Sir William Herschel determined the ellipticity of *Saturn* to be $\frac{1}{10{\cdot}368}$ (*Phil. Trans.* 1806, p. 461), Bessel found it to be $\frac{1}{10{\cdot}199}$ (*Ast. Nach.* No. 275, p. 170). The following is a synoptic view of the different results:—

Main	0·10838	=	$\frac{1}{9{\cdot}227}$
Johnson	0·09940	=	$\frac{1}{10{\cdot}060}$
Bessel	0·09805	=	$\frac{1}{10{\cdot}199}$
Herschel I.	0·09630	=	$\frac{1}{10{\cdot}384}$
Thompson	0·08902	=	$\frac{1}{11{\cdot}233}$
Dawes	0·08341	=	$\frac{1}{11{\cdot}988}$
De la Rue	0·08139	=	$\frac{1}{12{\cdot}29}$
Lassell	0·07876	=	$\frac{1}{12{\cdot}697}$

* The letter from Mr. Dawes to Mr. De la Rue was a *private* one, and should have been referred to the writer *before printing*, but was not, either from hurry or inadvertence.—R. SHEEPSHANKS.

resemblance to a fan. On the 11th of October M. Fearnley perceived a short ray of light, about 1′ in length, issuing from the nucleus in the direction of the tail. It is worthy of remark, that this singular appearance was witnessed on the same night by Mr. Hind, at the observatory of Mr. Bishop, Regent's Park (*Ast. Nach.* No. 839, p. 371). It may be mentioned, also, that phenomena of the same nature were perceived by Kepler in the great comet of 1618 (*De Cometis*, p. 103), and by Sir John Herschel in Halley's Comet (*Ast. Obs. at the Cape of Good Hope*, p. 396). The comet at one time was distinctly visible to the naked eye. It was so seen by M. Reslhuber on the 7th of October. It ceased to be generally visible even in telescopes before the middle of December.* Only *one* observation of a later date exists, so far as is hitherto known, viz. the observation by Sig. Secchi already referred to.

Ascensions droites relatives des 36 Etoiles fundamentales, déduites des Observations faites à l'Observatoire Royal de Greenwich depuis 1750 jusqu'en 1762, et depuis 1836 jusqu'en 1850. (Second Partie.) Par M. U. J. Le Verrier.

(*Comptes Rendus, Dec.* 6, 1852.)

In the *Comptes Rendus* for the 5th of April, 1852, there is a brief account of the first part of this important memoir. In the December number M. Le Verrier gives the corrections which should be applied to the right ascension of each star (as computed in the *Tabulæ Regiomontanæ*) for every ten years from 1750 to 1850. These corrections are deduced by the aid of two Greenwich catalogues, one of which is founded on the observations of Bradley and the other on the modern observations extending from 1836 to 1850, executed under the superintendence of the present Astronomer Royal.

One of the most interesting results of M. Le Verrier's researches is the establishment of the *variability* of the proper motion of *Sirius*. This remarkable fact, the original discovery of which is due to Bessel, although its existence has been subsequently disputed, is clearly indicated by the observations of Bradley, and still more so by those upon which the catalogue of 1850 is founded. It appears, also, that the corrections to the place of the star agree both in quantity and direction with the hypothesis which supposes the star to revolve round a large opaque body. This hypothesis more especially accounts for the positions of *Sirius* from 1830 to 1850 with the most perfect accuracy. Indications of variability are also perceptible in the proper motion of *Procyon*, but the results in this case are not sufficiently unequivocal to justify a definitive conclusion on the subject. The following are the corrections above mentioned, which are to be applied according to their signs to the data of the *Tabulæ Regiomontanæ*:—

* M. Fearnley's observations of the comet come down to Dec. 6; M. Reslhuber's, to Dec. 10; and Mr. Hartnup's, of the Liverpool Observatory, to Dec. 12 (*Ast. Nach.* No. 840, p. 393).

rections of the relative Right Ascensions of the 36 Fundamental Stars comprised in the Tabulæ Regiomontanæ.

(Expressed in thousandths of a second of time.)

	1750	1760	1770	1780	1790	1800	1810	1820	1830	1840	1850
si.....	+108	+ 90	+ 72	+ 53	+ 34	+ 13	— 8	— 28	— 49	— 70	— 92
is.....	— 46	— 35	— 24	— 13	— 3	+ 8	+ 18	+ 27	+ 36	+ 44	+ 53
.....	+ 11	+ 12	+ 13	+ 14	+ 15	+ 16	+ 17	+ 17	+ 17	+ 17	+ 17
ran ...	— 26	— 17	— 8	0	+ 9	+ 18	+ 28	+ 37	+ 46	+ 56	+ 66
ı	— 24	— 23	— 21	— 19	— 15	— 9	— 1	+ 7	+ 16	+ 26	+ 37
.....	+ 51	+ 49	+ 47	+ 45	+ 43	+ 40	+ 38	+ 36	+ 33	+ 30	+ 27
i	+163	+142	+121	+101	+ 81	+ 62	+ 42	+ 23	+ 5	— 13	— 30
ıis	+ 42	+ 38	+ 33	+ 29	+ 25	+ 21	+ 17	+ 13	+ 10	+ 7	+ 3
.....										+263	+114
.....	— 61	— 43	— 24	— 5	+ 14	+ 34	+ 54		+353	+373	+394
n.....	— 58	— 57	— 55	— 53	— 50	— 46	— 42	— 37	— 31	— 26	— 20
.....	—112	— 94	— 75	— 56	— 37	— 18	+ 1	+ 20	+ 40	+ 61	+ 82
æ	—101	— 82	— 63	— 45	— 27	— 9	+ 9	+ 27	+ 44	+ 61	+ 78
s.....	—122	—102	— 82	— 63	— 45	— 27	— 9	+ 9	+ 26	+ 43	+ 59
is	—106	— 87	— 68	— 50	— 32	— 15	+ 2	+ 18	+ 34	+ 49	+ 64
nis ...	+ 9	+ 13	+ 18	+ 22	+ 27	+ 31	+ 35	+ 38	+ 40	+ 42	+ 44
nis ...	— 19	— 19	— 19	— 19	— 19	— 19	— 20	— 21	— 23	— 25	— 28
ıs	— 34	— 23	— 13	— 4	+ 4	+ 11	+ 16	+ 20	+ 22	+ 23	+ 23
æ.....	+136	+120	+103	+ 85	+ 66	+ 48	+ 29	+ 9	— 10	— 30	— 49
æ.....	+228	+200	+172	+144	+115	+ 87	+ 58	+ 29	0	— 30	— 60
næ ...	— 32	— 25	— 17	— 9	— 1	+ 6	+ 11	+ 17	+ 24	+ 30	+ 36
ntis ..	+165	+142	+118	+ 94	+ 70	+ 46	+ 21	— 3	— 28	— 53	— 78
s	+106	+ 89	+ 72	+ 56	+ 39	+ 22	+ 5	— 12	— 28	— 45	— 62
ulis ...	— 56	— 52	— 47	— 43	— 39	— 35	— 32	— 29	— 26	— 22	— 19
achi...	—141	—114	— 86	— 59	— 32	— 6	+ 20	+ 46	+ 71	+ 97	+122
.....	— 52	— 46	— 40	— 33	— 27	— 21	— 15	— 9	— 3	+ 3	+ 8
læ	+176	+151	+126	+101	+ 75	+ 50	+ 24	— 2	— 28	— 54	— 81
læ	— 12	— 12	— 13	— 13	— 14	— 15	— 16	— 17	— 18	— 18	— 18
læ	+152	+128	+102	+ 75	+ 48	+ 21	— 7	— 35	— 64	— 93	—123
icorni	— 51	— 49	— 46	— 44	— 42	— 40	— 38	— 36	— 35	— 33	— 32
icorni	+ 20	+ 11	+ 3	— 6	— 15	— 25	— 35	— 44	— 54	— 64	— 74
i	—110	— 96	— 82	— 68	— 55	— 42	— 30	— 18	— 6	+ 6	+ 17
rii ...	— 97	— 87	— 77	— 68	— 59	— 50	— 41	— 33	— 25	— 17	— 10
aut ...	— 58	— 69	— 81	— 94	—107	—121	—135	—149	—163	—177	—191
i	+ 6	+ 7	+ 8	+ 8	+ 8	+ 8	+ 7	+ 5	+ 3	+ 2	0
omedæ	— 13	— 4	+ 5	+ 14	+ 22	+ 30	+ 37	+ 44	+ 51	+ 57	+ 62

n 1750 to 1810 the correction for *Castor* refers to the centre of light of the two stars. From 1830 to 1850 it refers to the position of the *second* star.

Table of the Correction peculiar to the Star Sirius.

	s		s		s
1750	+0·219	1760	+0·005	1842	+0·286
1751	·185	1761	·001	1843	·279
1752	·153	1762	·000	1844	·261
1753	·123			1845	·236
1754	·096	1836	·162	1846	·210
1755	·073	1837	·187	1847	·184
1756	·053	1838	·213	1848	·159
1757	·036	1839	·239	1849	·136
1758	·023	1840	·263	1850	+0·114
1759	+0·013	1841	+0·280		

A curious anomaly offered itself to M. Le Verrier in the course of his researches on Bradley's observations. The differences of the mean right ascensions of stars distant about twelve hours from each other were found to be subject to a small periodic variation, which appeared to pass through the cycle of its values in the course of every twelve months. This variation amounted at its maximum to 0ˢ·06. M. Le Verrier seems disposed to attribute it to an imperfect compensation of the pendulum. This circumstance recalls to mind an idea formerly broached by Mr. Sheepshanks, viz. that of dispensing with all mechanical contrivances for compensation, by placing the pendulum at such a distance below the surface of the earth as to render it inacessible to rapid fluctuations of temperature. A similar mode of getting rid of the consequences of imperfect compensation has been recently proposed by M. Faye, who justly remarks, that the invention of the electric telegraph suggests a method of peculiar facility for reducing it to practice.

But it may be mentioned, that the modern Greenwich Observations have not offered to M. Le Verrier any trace of the anomaly above referred to.

METIS.

Professor Wolfers, of Berlin, has investigated new elements of this planet, which he has employed in calculating an ephemeris for the opposition of 1853. The ephemeris extends from Sept. 17 to Oct. 31 (*Ast. Nach.* No. 845, p. 75).

THETIS.

An ephemeris of this planet has been calculated by Dr. Hartwig, of Leipsic, for the opposition of 1853. It extends from April 16 to August 30 (*Ast. Nach.* No. 847, p. 103).

CALLIOPE.

An ephemeris for the opposition of 1853, extending from Feb. 1 to April 6, has been calculated by M. Hornstein, of the Vienna Observatory (*Ast. Nach.* No. 847, p. 111).

On the Variable Stars S Cancri and Algol. By Professor Argelander (*Ast. Nach.* No. 845).

M. Argelander has recently been devoting attention to the star *S Cancri*, which has been found to undergo periodical fluctuations of brightness analogous to those which distinguish the well-known variable star *Algol*. On the 19th of December, 1852, he perceived the star when its light was very faint. The interposition of clouds prevented him from observing the precise instant of minimum brightness, and when the sky again cleared up, the light of the star had already sensibly increased. He is of opinion, however, that the period of variation as ascertained by him, viz. $9^d\ 11^h\ 37^m$, cannot differ more than two minutes from the true value. All his observations of the star since February 16, 1850, agree very well with this determination; but he has not yet succeeded in observing an exact minimum. As the star affords a visible minimum only every nineteen days, there is little hope of a single individual accomplishing much by means of such observations, and it were therefore desirable that other astronomers should direct their attention to this remarkable object. A 5-foot Fraunhofer, or even a 4-foot instrument with a dark sky, will suffice for this purpose, and the star is easy to find by means of its situation with respect to *Præsepe* and *δ Cancri*. The following are the positions of the star, and also those of the stars of comparison for the mean equinoxes of 1800 and 1850.

		1800. R.A.	1800. Decl.	1850. R.A.	1850. Decl.
		h m s	° ′	h m s	° ′
Var.	*S.*	8 32 30	+19 44	8 35 22	+19 34
8 to 8·9	*a*	34 20	+19 46	37 12	+19 36
9	*b*	31 40	+19 46	34 32	+19 36
8	*d*	30 44	+19 38	33 36	+19 28
10	*e*	34 15	+19 35	37 7	+19 25

The variable star, and also *a* and *b*, are to be found in Bessel's Zone, No. 277. Lalande has observed each of the stars *a* and *d* twice; *e* is estimated by the eye; it forms with *b* and *S* nearly a right-angled isosceles triangle, the right angle being at *S*. The visible minima for the month of March are,

March 5 ..... 7^h
— 24 6

M. Argelander recommends to those astronomers who may be desirous of aiding in such observations, to compare the variable star with either of the standard stars, by bringing them one after the other into the *centre of the field of view*, and not to observe both objects of comparison with the telescope in a fixed position, since the star which stands out from the centre always appears *too bright*. When it attains its minimum, *S* will be very nearly equal in brightness to *e*. M. Argelander remarks, that it begins to increase very rapidly; and he is inclined to suppose that its descent from the maximum is even still more rapid.

The following table of the minima of *Algol*, which are visible in Europe during the present year, has been calculated by M. Argelander.

Visible Minima of Algol in the Year 1853.

Bonn M.T.

		h	m			h	m			h	m
Jan.	6	20	20	May	21	14	38	Oct.	3	8	56
	9	17	9		24	11	27		6	5	45
	12	13	58		27	8	16		17	17	1
	15	10	46	June	10	16	20		20	13	50
	18	7	35		13	13	9		23	10	39
	21	4	24		16	9	58		26	7	28
	29	18	51	July	3	14	52		29	4	16
Feb.	1	15	40		6	11	40	Nov.	6	18	43
	4	12	29		9	8	29		9	15	32
	7	9	18		23	16	34		12	12	21
	10	6	7		26	13	23		15	9	10
	21	17	22		29	10	12		18	5	59
	24	14	11	Aug.	1	7	1		26	20	25
	27	11	0		15	15	5		29	17	14
March	2	7	49		18	11	54	Dec.	2	14	3
	16	15	53		21	8	43		5	10	52
	19	12	42	Sept.	4	16	47		8	7	41
	22	9	31		7	13	36		11	4	30
April	8	14	25		10	10	25		19	18	57
	11	11	13		13	7	14		22	15	46
	14	8	2		24	18	30		25	12	34
	28	16	7		27	15	19		28	9	23
May	1	12	56		30	12	7		31	6	12
	4	9	45								

Elements of Comet III. 1847, *deduced from the totality of the observations* (*Ast. Nach. No.* 845, p. 77).

This comet, which was discovered by M. Mauvais on the 4th of July, 1847, is remarkable for having continued visible till the month of April, 1848: M. Gautier, having calculated elliptic elements from three observations, found the time of a complete revolution to be 31,500 years. This result induced him to confine his attention to the investigation of such elements as would best represent the motion of the comet in any assigned part of its course. To this end he corrected the elements by means of the totality of the observations, taking into account the perturbations of *Jupiter*, *Venus*, and the earth; and then compared the results of this correction with eight normal positions of the comet, deduced from the observations. The agreement, except in one or two instances, is sufficiently satisfactory.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIII. *Feb.* 11, 1852. No. 4.

THE Annual General Meeting of the Society, J. C. ADAMS, Esq., President, in the chair.

Rev. J. V. Mummery, Richmond Grove, Canonbury;
Rev. W. Giles, Chester; and
Rev. Josh. Woolley, LL.D., H.M. Dockyard, Portsmouth;
were balloted for and duly elected Fellows of the Society.

Report of the Council to the Thirty-third Annual General Meeting.

The Council meet the Society again with pleasure, in the accounts they are able to give of the past year and of the prospects of astronomy for the future. With the exception only of those parts of their Annual Report which must always be brought forward with regret, the obituary record, and notice of retirements, or other loss of services, there is nothing on which they may not dwell with satisfaction. But the Society has now attained an age which, in the course of nature, makes it necessary to record the deaths of its oldest members more frequently than hitherto, and the present Report must be marked by its notice of the loss of General Colby, one of our oldest Fellows and most familiar names.

The Report of the Auditors, subjoined, will show the state of the finances:—

RECEIPTS.

	£	s.	d.
Balance of last year's account	276	0	11
By dividend on £2512 14*s.* 11*d.* 3¼ per Cents	39	12	10
By ditto on £1150 Consols	16	15	0
By ditto on £2575 18*s.* 9*d.* 3¼ per Cents	40	12	9
By ditto on £1150 Consols	16	15	0
On account of arrears of contributions	119	14	0
116 contributions (1852–53)	243	12	0
4 ditto (1853–54)	8	8	0
5 compositions	105	0	0
14 admission fees	29	8	0
9 first year's contributions	14	14	0
Sale of Publications	63	9	6
	£974	2	0

EXPENDITURE.

	£	s.	d.
Cash paid for investment of compositions of Rev. T. W. Webb, H. L. Pattinson, Esq., and Rev. T. A. Southwood	63	0	0
Mr. Basire, engraver	19	19	0
Mr. Wyon (medals)	57	15	0
George Barclay (printer)	127	1	1
Taxes { 1 year's income tax 1 9 2 / 1 year's land tax 4 15 10	6	5	0
J. Williams, for sundry disbursements, as per accounts Nos. 1, 2, 3, 4	99	15	10
Ditto for salary	100	0	0
Ditto commission on collecting £479 5s. 6d.	23	19	6
Balance in Treasurer's hands	476	6	7
	£974	2	0

Assets and present property of the Society:—

	£	s.	d.
Balance in the hands of the Treasurer	476	6	7
Due from Mr. Byng £24 8 0			
2 contributions of 7 years' standing 29 8 0			
4 ——— of 5 ditto 33 12 0			
2 ——— of 4 ditto 16 16 0			
5 ——— of 3 ditto 31 10 0			
6 ——— of 2 ditto 25 4 0			
17 ——— of 1 ditto 35 14 0	196	12	0
Due for publications of the Society	3	6	0

£1150 3 per Cent Consols.
£2575 18s. 9d. 3¼ per Cent Annuities.
Unsold publications of the Society.
Various astronomical instruments, books, prints, &c.

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	40	VII.	223	XVII.	253
I. Part 2	83	VIII.	210	XVIII.	270
II. Part 1	102	IX.	218	XIX.	289
II. Part 2	63	X.	228	XX.	304
III. Part 1	130	XI.	238		
III. Part 2	150	XII.	247		
IV. Part 1	152	XIII.	264		
IV. Part 2	165	XIV.	451		
V.	180	XV.	277		
VI.	198	XVI.	263		

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1852	134	178	65	6	383	58	441
Since elected	3	11	...	...	14	...	14
Deceased	−6	−3	−1	...	−10	...	−10
Removals	2	−2	...	...	...	...	...
February 1853	133	184	64	6	387	58	445

The instruments belonging to the Society are now distributed as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Owen* portable quadruple sextant,
The *Beaufoy* circle,
The *Herschelian* 7-foot reflector,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,

are in the apartments of the Society.

The Brass quadrant, said to have been *Lacaille's*,

is in the apartments of the Royal Society.

The Standard scale

is in the charge of the Astronomer Royal, with the consent of the Council, to be employed in the construction of a new Standard Measure, under the direction of the Standard Committee.

The remaining instruments are lent, during the pleasure of the Council, to the several parties undermentioned, viz.:

The *Beaufoy* clock,
The two invariable pendulums, } to the Royal Society.
The *Wollaston* telescope, to the representatives of the late Professor Schumacher.
The other *Beaufoy* clock, to Dr. Drew.
The *Lee* circle, to Captain Blackwood.
The Variation transit (late Mr. Shearman's), to Mr. Gravatt.

EXPENDITURE.

	£	s.	d.
Cash paid for investment of compositions of Rev. T. W. Webb, H. L. Pattinson, Esq., and Rev. T. A. Southwood	63	0	0
Mr. Basire, engraver	19	19	0
Mr. Wyon (medals)	57	15	0
George Barclay (printer)	127	1	1
Taxes { 1 year's income tax ..... 1 9 2 / 1 year's land tax 4 15 10	6	5	0
J. Williams, for sundry disbursements, as per accounts Nos. 1, 2, 3, 4	99	15	10
Ditto for salary	100	0	0
Ditto commission on collecting £479 5*s.* 6*d.*	23	19	6
Balance in Treasurer's hands	476	6	7
	£974	2	0

Assets and present property of the Society:—

	£	s.	d.
Balance in the hands of the Treasurer	476	6	7
Due from Mr. Byng £24 8 0			
2 contributions of 7 years' standing 29 8 0			
4 ——— of 5 ditto 33 12 0			
2 ——— of 4 ditto 16 16 0			
5 ——— of 3 ditto 31 10 0			
6 ——— of 2 ditto 25 4 0			
17 ——— of 1 ditto 35 14 0			
	196	12	0
Due for publications of the Society	3	6	0

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but strictly scientific astronomical information. In other respects no change is intended.

First, it is proposed to insert notices and abstracts of valuable astronomical papers contained in the *Astronomische Nachrichten*, the *Comptes Rendus*, and other periodicals, including the Memoirs of different Societies at home and abroad.

Secondly, to give the accurate titles of new books of value and interest which have been presented to the Society, or which can be carefully examined. When practicable, such an account will be given of these works as may enable any one to form an opinion whether they are likely to suit him, and frequently this may be done in a few words.

Thirdly, but this must depend upon the co-operation of the Fellows, notices will be given of older works, which, from their importance and rarity, deserve or require particular attention.

Notwithstanding the zeal and extraordinary qualifications of Mr. Grant, it is scarcely possible that all these objects should be satisfied, unless the members of the Society will assist a little, each in his own walk; but with this help it may reasonably be expected that the *Monthly Notices* will fill up a void which is felt by the astronomers of England, and probably by those of most other countries, while they will avoid any unseemly rivalry with other publications.

The Council cannot lose the services of Mr. Sheepshanks, though they trust the loss will only be temporary, without expressing their sense of the very great benefit the Society has derived from them, both as Editor of the *Monthly Notices* and as an active member of the Council. The change in the *Monthly Notices*, from no more than a record of what passed at the ordinary meetings, into a publication of the character of transactions, but of much more frequent appearance, and more rapid in its announcements, is entirely due to him. And though the Council feel that the residence of so energetic a member abroad will materially tend to increase the connexion of the Society with the part of Europe in which he resides, to the advantage of astronomy both here and there, yet they do not accept this benefit as an adequate compensation for the loss of services which, always of value, were of the utmost importance since the greatest crisis of the Society's affairs — that caused by the death of Mr. Baily.

It may seem useless, after what has just been said, to call particular attention to the *Astronomische Nachrichten*, but the importance of that publication to the progress of our science can scarcely be estimated too highly, or too often insisted upon. Besides researches of the highest value by the most eminent geometers of the Continent, the amateur astronomer will find a most complete collection of observations and ephemerides, and other information which he may require. These are quite intelligible to a person not acquainted with German, even when that language is used; but an Englishman will find a good deal in his own tongue, either of English or American produce, and may not be displeased to learn that the

honour of his race is so well supported. The subscription is so reasonable and so easily paid, and the work itself is of such vital importance, that it may be safely recommended to the support of lovers of science generally, upon that account alone. But every one who is either an observer or computer will find a sufficient motive in the work itself, and in his own gratification and instruction. The *Nachrichten* and *Monthly Notices* combined should find an intelligent amateur a good deal to do, and a good deal to think upon, and jointly they would keep him pretty well up with the advance of the science.

It may be well to remind gentlemen who have imperfect copies of this work, that no time should be lost in giving notice of their wants. A list will be sent very shortly to Altona of these lacunæ; and it will be difficult, if this opportunity be neglected, to supply them hereafter.

The Council have awarded the gold medal to Mr. J. R. Hind, for his astronomical discoveries, and in particular for the discovery of eight small planets. It is true that, at the time when the proposition to award this medal was first entered on the minutes of the Council, the number of planets discovered was but six; nevertheless, looking at the circumstance that the merit of this discovery lies in the skill and perseverance with which the systematic examination of the heavens was made, the Council feel justified in making the terms of the announcement contain the number of proofs of success extant at the time when the final award was made. The President will detail the grounds of the award at the close of the meeting.

The Council have to regret the loss, by death, of the following Fellows: — W. T. Clark, Esq.; Major-general Sir T. Colby; S. E. Cottam, Esq.; Geo. Dollond, Esq.; Rev. J. Edmonstone; Thos. Jones, Esq.; Lieut. Lecount; T. Norris, Esq.; Rev. Chas. Turnor; and J. Vandercom, Esq.

William Tierney Clark, Esq. was born on the 23d of August, 1783, at Sion House, in Somersetshire. He commenced his career under a millwright at Bristol, to whom he was apprenticed when very young for several years. From Bristol he went to Coalbrook Dale, where he continued till the year 1808, when he went to London, and was shortly after, at the request of his former employers, admitted as a draftsman into the employment of the late Mr. Rennie, with whom he remained till 1811, when he was appointed engineer of the West Middlesex Waterworks. At that period the West Middlesex Waterworks were in a comparatively insignificant state. They had been established upon a very limited scale, and the greatest capabilities of the works were confined to supplying water to the neighbouring hamlets by means of a small pumping-engine of twenty horse power, and a small reservoir, barely sufficient to contain a day's supply. The West Middlesex

Waterworks had only been established in 1806; they were then unremunerative, nor did they yield any dividend until several years after Mr. Clark became manager of them. Mr. Clark, however, was not disheartened: on the contrary, he encouraged his employers to make new sacrifices, by increasing the amount of their capital and expending it judiciously in increasing the works. He foresaw the prosperity which would ere long attend those sacrifices, and he spared no exertion to render the works as perfect as possible, by increasing the number and power of the engines and the capacity of the reservoirs; so that, from the insignificant establishment of 1819, based upon a capital of 20,000*l.*, producing no revenue, he left the works at his death increased to three large pumping-engines of the aggregate power of 245 horses; two capacious reservoirs, capable of containing between thirty-five and forty millions of gallons, and of supplying an abundant quantity of good water to the extent of one-tenth of the whole supply of the metropolis, with a capital of nearly 700,000*l.*, and an annual rental of nearly 70,000*l.* per annum. Such was, in a great measure, the result of Mr. Clark's labours.

The first work of civil engineering undertaken by Mr. Clark was the Thames and Medway Canal. That work had been commenced by a company of shareholders, for the purpose of profiting by the advantages which they expected to derive from shortening the navigation between Gravesend and Chatham forty miles, by means of a cut across the isthmus which separates the Thames and the Medway. The project had been stopped for want of capital, but it was again resumed under Mr. Clark's direction in 1819, but was not completed till several years afterwards. The canal was principally remarkable from the great length and size of the tunnel, and which, in carrying through the Frinsbury ridge of hills near the Medway, occasioned great difficulty and expense. This canal is now converted into a railway.

Mr. Clark's next work was the Hammersmith Suspension Bridge. It was commenced in 1824, and finished in 1827. Its dimensions have been described elsewhere; suffice it to say, that it is principally remarkable for the small deflexion of the chains between the chord line or points of suspension, being 422·3, the versed sine is only 29·6 or $\frac{1}{14}$.

The Marlow Suspension Bridge over the Thames was originally designed and commenced by his predecessor in the West Middlesex Waterworks, Mr. Millington, in 1829, but was completed after Mr. Clark's own design. The bridge resembles in its style and construction that of Hammersmith, with slight difference in the dimensions and in the architecture of the piers and suspending towers.

The same may be said of the Norfolk Suspension Bridge, which he erected for the Duke of Norfolk, near Shoreham, over the Arun. The style of architecture is more decorative and elegant.

Mr. Clark erected about this period a handsome cast-iron bridge at Bath, also a small cast-iron bridge at Windsor.

But the finest work of Mr. Clark is the Pesth Suspension Bridge over the Danube. The origin and progress of this magnificent undertaking will be found in an octavo volume recently published by Mr. Weale, under Mr. Clark's directions. It contains a translation of the Reports of Counts George Andrassy and Stephen Széchenyi on the expediency of constructing a bridge over the Danube between the cities of Pesth and Buda; their questions to and answers from Mr. Clark; the memoir of Count Széchenyi; a Report of the Ban Direction, or Board of Bridges and Roads; and a detailed statement by Mr. Clark himself of the progress of the works of the bridge, from their commencement in 1839 to their completion in 1849: from which it appears that the total cost of the bridge, without deducting the old materials, was 622,042*l.*

In 1845 Mr. Clark sent over to St. Petersburg a magnificent design for a suspension bridge over the Neva. It was greatly admired by the Emperor, who in return presented him with a first-class gold medal.

The Emperor of Austria also presented Mr. Clark, through the Archduke Charles, with a splendid box set in brilliants, as a token of his approbation on the completion of the Pesth Suspension Bridge.

"Thus," says Mr. Clark, "was finished Pesth Suspension Bridge, a work which in its construction encountered probably more difficulties than any structure of a similar kind in existence. The magnitude of the river over which it is thrown, the depth and nature of its bed, and velocity of the current, created the misgivings, at one time almost universal in Hungary, that no permanent communication could ever be established across the Danube between Buda and Pesth. The moral difficulties to be overcome, no less than the physical obstacles, were very great. Pride, prejudice, and jealousy, had each to be encountered; so universal against the object at one period, that nothing less than the extraordinary energy and perseverance shown by Count Széchenyi could have withstood their evil effects, and few would have made the attempt."

Our limits preclude our entering into the difficulties which the bridge encountered from the bursting of the dams, the shocks of the ice, or the ravages of hostile armies. The physical difficulties were all encountered and overcome by the energy and perseverance of Mr. Clark and his assistants. The Suspension Bridge of Pesth while it stands, a monument to his genius, is the admiration of all who have seen it. It was the last and crowning act of a life devoted to a profession of which he was an ornament. He died on the 22d of September last, aged sixty-nine, deeply regretted by all who were intimate with him. His remains were consigned to a family vault in Hammersmith church.

Thomas Colby, born at Rochester on the 1st September, 1784, was a son of Major Colby of the Royal Marines, who served during the war, and was in several engagements under Lord Howe and other naval commanders; he had four uncles in

the army, one of whom, General Hadden, was Surveyor-general of the Ordnance, and another, Colonel Hadden of the 11th Regiment, was Paymaster-general of the Forces in Portugal. Thomas Colby was educated at Northfleet, Kent, by the late Dr. Crackell, a man fully capable of appreciating the superior abilities of his pupil; from this school he proceeded to the Royal Military Academy at Woolwich, and after having been selected for, and appointed to, the corps of Royal Engineers, was placed under Mr. Robert Dawson, of the corps of Royal Military Surveyors and Draftsmen, then employed on the Ordnance Survey of Cornwall, for the purpose of being instructed in surveying and military drawing. At this time he met with an accident, by the bursting of a pistol, which deprived him of his left hand, and very nearly cost him his life, as a part of the barrel entered his head. This accident had an important bearing on his future career: the loss of the hand was a serious hindrance and objection to military service, but Major Mudge, the Superintendent of the Trigonometrical Survey, knowing Lieut. Colby's qualifications, with great kindness and consideration applied immediately to the Master-general of the Ordnance, that he might be permanently attached to the survey as an assistant to himself, in the same way that he had formerly been to his predecessor, Lieut.-colonel Williams, R.A.

Having been appointed second lieutenant on the 21st December, 1801, when a little more than seventeen years of age, and posted to the survey in January 1802, he was for many years employed during the summer and autumn months in observing at various stations with the great theodolite and zenith sector (both constructed by Ramsden), for the principal triangulation and the measurement of the arc of meridian between the Shetland Isles and Isle of Wight; and during the rest of the year was engaged at the Drawing-room in the Tower of London, in roughly computing the results of the summer's observations and preparing them for publication, and in superintending the construction and engraving of the Ordnance Maps of England and Wales, on the scale of one inch to a mile, or $\frac{1}{63360}$: meanwhile, he was made a first-lieutenant in August 1802, and captain in July 1807.

He assisted, and was associated with, Lieut.-colonel Mudge, in preparing the third volume of *An Account of the Trigonometrical Survey of England*, which was published in 1811; and on Major-general Mudge's death, in 1820, he was appointed by his Grace the Duke of Wellington to succeed him as Superintendent of the Trigonometrical Survey, having been strongly recommended by the President of the Royal Society, Sir Joseph Banks, to whom the Duke referred for his opinion of Captain Colby's scientific qualifications for the appointment.

He obtained his majority by brevet in July 1821, and in this and the two following years we find him associated with Captain Kater, appointed by the Royal Society to co-operate with MM. Arago and Mathieu, acting in behalf of the Institute of France, in the verification of the connexion between the observatories of

Greenwich and Paris; the English portion of the triangulation for this purpose had been originally executed by Major-general Ross, and was now revised by Captain Kater and Major Colby, who both personally assisted in making the observations; and Major Colby afforded the aid of Mr. James Gardner, one of the most experienced observers on the survey.

In 1824 the survey of Ireland was commenced, after a careful investigation before a select committee of the House of Commons as to the best method of proceeding with such survey, its nature, scale, and the details it should embrace; and on the recommendation of that committee its execution was entrusted to the Ordnance Department, and confided by the Master-general of the Ordnance, the Duke of Wellington, to Major Colby's charge. This survey being intended to facilitate a general valuation of property throughout Ireland, with a view to apportioning more equally the local taxation, was required to be so precise that the accuracy of its details should be unquestioned; on the other hand, the cost of the survey was to be kept within reasonable limits.

Major Colby, therefore, wisely determined not to adopt the system of working by contract, then followed in England, in the preparation of the Ordnance Map of England and Wales, but to make the survey of Ireland depend on actual measurements with the chain, based on a very complete principal, secondary, and minor triangulation, carried out so minutely as to include the fixation of a trigonometrical point for about every 400 acres. In executing this plan he devised and arranged so complete a system of operations, and subdivided the survey into so many distinct branches, proceeding from, but serving as a check on, each other, that he was ultimately enabled to avail himself of the natural aptitude and intelligence of the lower orders of the Irish to form the great mass of the *employés* on the survey, whose number at one period exceeded 2000 persons, mostly from the native population.

In 1825 Major Colby obtained the Duke of Wellington's permission to have three companies of Royal Sappers and Miners raised and trained to assist in carrying on the survey of Ireland, so as to make the work essentially military in its organization: the whole cost of these three companies, containing 105 men each, who could at once be made available for the ordinary military service of the country, was to be defrayed out of the parliamentary votes annually sanctioned for the purposes of the survey.

Major Colby's great difficulty at the outset consisted in the want of an adequate number of trained assistants; he had devised an entirely new system, which could not be practically carried out until he had trained his assistants to the routine of the various processes. Accordingly, the progress of the work at first appeared to be slow and unsatisfactory, and a proposition was made in the year 1828 to introduce a less rigorous but more rapid mode of procedure, and a committee of officers of Engineers, under the presidency of Master-general Sir J. C. Smyth, Bart., was appointed by the Master-general of the Ordnance (Lord Beresford) to inves-

tigate the whole subject; and the result of their labours was, the introduction of less accurate methods, which, however, were cancelled in the course of a few years, and Colonel Colby was permitted to proceed with the work as he originally intended.

Another change was also made in the year 1828. Till then the whole of the correspondence passed direct from the Master-general and Board of Ordnance to the Superintendent of the survey, and Colonel Colby's views on all subjects were explained, either verbally or by correspondence, to the Master-general or individual members of the Board: the sole responsibility for the manner in which the work was carried on thus rested on the superintendent. The change consisted in placing the survey branch of the Ordnance, which Colonel Colby had with so much labour to himself succeeded in forming, immediately under the control of the Inspector-general of Fortifications; so that all reports of progress, applications for increased powers, &c., had to be transmitted to the Board of Ordnance through that officer, as the head of the corps of Royal Engineers. Colonel Colby did not consider the change to be productive in any way of benefit to the public service.

The Report of the Select Committee of the House of Commons had recommended that the survey should be limited to ascertaining and laying down the boundaries of townlands, the roads, streams, and houses, and that it should not embrace the insertion of the fields; but after many counties in the north of Ireland had been completed in this manner, the Government Commissioner for the general valuation reported that the detail thus supplied to him was insufficient, and that he required the principal farms and fields to be shown. In consequence of this representation, all the fields and enclosures were inserted in the maps of the remaining counties; and we understand that the maps of the northern counties, thus left incomplete in the first instance, are now being gradually revised and corrected to make them as perfect as the others.

The engraving of the 6-inch map of Ireland was not originally contemplated by the Select Committee of the House of Commons, but occurred in this manner:—The Valuation Department, under a separate chief, appointed by the Irish Government, required to be supplied with six copies of the maps, and Colonel Colby having ascertained that the cost of engraving the maps on copper would not be likely to exceed the cost of making these six copies, the Irish Government, therefore, determined to obtain a yearly grant of money from Parliament, and to have the maps engraved; so that, at the same cost as would have been necessary for the general valuation to be properly made, the public have been put in possession of the most perfect topographical map, taken as a whole, of any in existence.

In 1828 Colonel Colby selected Major (then Lieut.) Larcom, R.E., as the executive officer for the Irish Survey, head-quarter office at Mountjoy Barracks, in the Phœnix Park, Dublin, to superintend the engraving, to ensure the various branches of the work being carried on in comple unison with each other, and to collect a perfect

record of everything that occurred, so that every document required to establish the authenticity of the work should be carefully preserved; and it is doubtful if anything could excel the admirable manner in which all these objects were effected.

The preliminary difficulties being once got over, and the injudicious attempt to substitute speed for accuracy finally abandoned in 1832, the progress of the survey became considerably accelerated, and the publication of the first county (Londonderry), which took place in May 1833, was rapidly succeeded by that of others, as will be seen by reference to the following table:—

Names of Counties.	No. of Sheets of the Map, including an Index Map.	Date of Publication.
Londonderry	50	May 1833
Antrim	69	February 1834
Tyrone.....	69	November 1834
Down	58	May 1835
Fermanagh	44	October 1835
Armagh	33	February 1836
Monaghan	35	April 1836
Louth	26	July 1836
Donegal	111	October 1836
Cavan	45	January 1837
Meath	54	April 1837
Leitrim	39	June 1837
Longford	28	January 1838
Sligo	48	— 1838
Roscommon	57	May 1838
Westmeath	41	September 1838
Kildare	41	— 1839
Mayo	124	February 1840
Wicklow	48	May 1840
King's County	48	July 1840
Carlow.....	27	October 1840
Galway	138	June 1841
Queen's County.....	38	September 1841
Wexford	55	December 1841
Kilkenny.....	48	June 1842
Clare	76	December 1842
Waterford	41	January 1843
Tipperary	92	October 1843
Dublin.....	29	February 1844
Limerick.....	61	July 1844
Cork	154	August 1845
Kerry	112	November 1847
Total number of Sheets	1939	

Whilst Colonel Colby was occupied in devising and introducing those admirable arrangements, under which the several operations connected with the survey of Ireland were gradually transformed into a beautifully-organised department, he did not neglect the necessary steps for ensuring that the scientific results from the survey of the United Kingdom of Great Britain and Ireland, when finally completed and published, should fulfil the expectation expressed in the concluding paragraph of the Report of the Committee of 1824, viz.: "that the survey would be carried on with energy as well as with skill, and that it would, when completed, be creditable to the nation and to the scientific acquirements of the present age."

He had had considerable experience with Major-general Mudge in the measurement of bases with the steel chain, having assisted that officer at the measurement of the bases on King's Sedgmoor in 1802, and Rhuddlan Marsh in 1806, and conducted the measurement of that on Belhelvic Links near Aberdeen, in 1817, the details of which have not yet been published; and he was perfectly familiar with the methods followed and apparatus used for the like purpose in France, but he was not satisfied with either; he considered that the actual temperature of the measuring chain, in whatsoever manner it might be ascertained, would not be found to accord with the temperature of the air, and that when considerable changes of temperature occurred in short intervals of time, the length of the measuring chain was not the length actually due to the indicated temperature. He, therefore, instituted a series of experiments on the heating and cooling of metal bars, and the changes in this respect that could be produced on their initial states, by varying the nature of their coatings or outer surface, with the object of preparing a measuring rod that should not change its length with changes of temperature; and he eventually succeeded in having an apparatus constructed, consisting of a bar of iron and another of brass, joined together at the centre of their length into one compound bar, which he denominated a "compensation bar," and by which the distance between two dots near the outer extremities of these bars remained invariable in length during very considerable changes of temperature: he also had a microscopic apparatus fitted to similar compensation bars six inches long, so that, by leaving a space of six inches between the outer foci or compensation points of the two outer microscopes, all chance of contact of the ends of the adjoining measuring rods, whilst the operation of measuring was in progress, was altogether avoided.

This beautiful apparatus has been very successfully used in the measurement of a base about eight miles in length, under Colonel Colby's personal superintendence, on the south shore of Lough Foyle in the north of Ireland, during the years 1827–8;* by Mr. Maclear the eminent astronomer at the Cape of Good Hope, in

* See *An Account of the Measurement of the Lough Foyle Base, with its verification and extension by Triangulation*, published by order of the Board of Ordnance in 1847.

1840–1, in the measurement of a base eight miles in length, preparatory to the verification and extension of the Abbé La Caille's arc of meridian, which measurement Mr. Maclear thinks there is just ground for believing is scarcely erroneous to the amount of half an inch, the error being probably much less than this; and in the re-measurement of Major-general Mudge's base on Salisbury Plain, about seven miles in length, in the year 1849, under the direction of Captain Yolland, R.E., employed under the command of Lieutenant-colonel Hall, R.E., who succeeded Major-general Colby in the superintendence of the Ordnance Surveys in 1847; and most admirably in all respects does the apparatus appear to have answered the anticipations of its inventor. A similar set of compensation bars and microscopes, constructed by the same makers (Messrs. Troughton and Simms) for the Hon. East India Company, have been used by Lieutenant-colonel Everest, formerly Surveyor-general, in the measurement of several bases in India.*

Colonel Colby also caused to be constructed two 10-feet iron standard bars, supported on rollers at one-fourth and three-fourths of their length, to afford the means of securing a permanent record of the absolute length of the compensation bars, and the bases measured with them at 62° Fahrenheit; one of these standard bars being intended to be used with the measuring apparatus in the field, and the other to be kept for safety and for record at the head-quarters of the Trigonometrical Survey. He also provided a 6-inch brass standard scale, to be used in preserving a record of the length of the 6-inch microscopic interval between each pair of compensation bars. He also caused two 3-feet iron bars to be made, of a similar construction to the 10-feet iron standards, and compared by the late Lieutenant Murphy, R.E., assisted by the late Mr. F. Baily Professor (then Lieutenant) Johnson, R.N., and Mr. Donkin, jun., in March, 1834, with the Parliamentary Standard Yard, which was subsequently lost at the fire which burnt down the Houses of Parliament in October 1834: these bars and comparisons have since formed a portion of the evidence on which the Commissioners appointed to prepare a new standard have had to depend for the accurate restoration of the lost standard yard.

The observations for the principal triangulation in Ireland, with the exception of the stations connected with and in the immediate vicinity of the Lough Foyle Base, were almost entirely taken by Lieutenant-colonel (then Lieutenant) Portlock, R.E., who also had the charge of the secondary and minor triangulation, and the computations for the supply of distances and altitudes; but that officer was subsequently appointed to conduct the Geological Branch of the Survey, whilst its execution formed part of the duties entrusted to the Ordnance in Ireland.†

* See *An Account of the Measurement of Two Sections of the Meridional Arc in India, by Lieut.-colonel Everest*, &c., published in 1847.

† See *Report of the Geology of Londonderry and parts of Tyrone and Fermanagh*, published in 1843, by Captain Portlock, R.E.

At one period Colonel Colby had it in contemplation to make the Topographical Survey the basis for geological, statistical, and antiquarian researches and surveys, and to connect them with museums of economic geology, both in England and Ireland; but his opinions and efforts in this direction were overruled by the more limited views of the authorities under whom he served.*

When Colonel Colby had succeeded in perfecting all the arrangements connected with the Irish Survey, he next turned his attention to the steps required for improving the accuracy of the one-inch map of England and Wales. Of the work already executed at least two-thirds had been done under the contract system, the surveyors employed being paid at a certain rate for every square mile of ground, for which they prepared a finished manuscript plan; and as no means existed of checking the accuracy of such plans, except by examining the correctness of the detail on the ground, it was found that after the plans had been sent in to the Drawing-room in the Tower, as complete in all respects, very extensive examinations and revisions were generally necessary before they could with safety be traced and transferred to the copper-plates for engraving. Colonel Colby, therefore, decided on having the work surveyed in a more accurate and methodical manner, and a portion of the Eastern Counties and the district of country contained in sheets 79, 80, 81, 82, 87, 88, 89, and 90; comprising parts of Lancashire, Yorkshire, Cheshire, Derbyshire, and Nottinghamshire, have been executed in this manner, and these parts of the one-inch map will, in consequence, be found free from the errors that are occasionally detected in and detract from the value of some of the sheets of South Britain, which have not been revised since their first publication.

The following Table exhibits the order, with dates, in which sheets of the one-inch map were published after Captain Colby succeeded to the superintendence of the Trigonometrical Survey; the local charge of the work at the Drawing-room, during his absence on other duties, having been entrusted in succession to Lieutenant-colonel (then Captain) Mudge, son of the late Major-general Mudge, the late Lieutenant-colonel (then Captain) A. W. Robe, R.E., and Captain Yolland, R.E.

No. of Sheet.	Date of Publication.	No. of Sheet.	Date of Publication.	No. of Sheet.	Date of Publication.
7	August 1822	86	March 1824	43	September 1831
64	March 1824	34	August 1828	55	February 1832
65	— —	44	November 1828	42	September 1832
69	— —	13	May 1830	36	January 1833
70	— —	35	— —	61	January 1833
83	— —	37	— —	73	May 1833
84	— —	54	May 1831	45	October 1833
85	— —	41	June 1831	56	— —

* See *Ordnance Survey of the County of Londonderry—Memoir of the City and North-Western Liberties of Londonderry*, published in 1837.

No. of Sheet.	Date of Publication.	No. of Sheet.	Date of Publication.	No. of Sheet.	Date of Publication.
62	January 1834	49	August 1837	76	June 1840
58	July 1834	50	— —	2	July 1840*
57	October 1834	67	December 1837	82	— —
53	— —	66	January 1838	79	— —
46	December 1834	38	January 1838*	87	August 1841
63	June 1835	74	February 1838	78	September 1841
52	November 1835	68	May 1838	90	October 1842
71	February 1836	48	November 1838*	81	November 1842
51	June 1836	75	December 1838	40	February 1843*
60	— —	33	January 1839*	89	— 1844
72	— —	77	March 1839	88	March 1844
59	June 1837	80	May 1840	1	November 1844*

In 1833, we find that Sir Henry (then Mr.) De la Beche made a proposal to the Government to undertake the publication of a geological map of the counties of Devon and Cornwall, and after some consideration it was finally arranged that the Ordnance Survey Department, under Colonel Colby, in conjunction with Sir H. De la Beche, should undertake the work; the Ordnance obtaining the necessary funds from Parliament for its execution, and Sir H. De la Beche being responsible for the geological information engraved on the Ordnance maps under Colonel Colby's direction.

The connexion thus established continued up to the year 1845, when the geological survey was transferred to the department of Woods and Forests (a considerable extent of country besides the counties of Devon and Cornwall having, in the intermediate period, being geologised and published), and it has since received a very considerable increase of strength under its able chief; the engraving is still executed by the Ordnance, but the cost is charged against the Woods and Forests.

Towards the close of the year 1840 the Government, yielding to the urgent representations of various scientific bodies in England and Scotland, consented to extend the advantages which had been found to arise from the publication of the Ordnance Maps of Ireland on the enlarged scale of six inches to a mile $= \frac{1}{10560}$, to the six counties of England remaining unsurveyed, viz. Lancashire, Yorkshire, Westmoreland, Cumberland, Durham, and Northumberland, and to the whole of Scotland, by ordering the survey of those parts to be conducted, and the maps to be engraved on that scale, the publication of the one-inch map for the remainder of England being still continued; and as the memorialists who had brought the subject before the Government had stated the necessity for showing the physical features of the country on the large-scale map, Colonel Colby proceeded to make the requisite arrangements for exhibiting this information by a series of contour or level

* Revised and re-engraved.

lines vertically equidistant from each other, the feasibility of such an undertaking at a moderate cost having been previously ascertained, at Major Larcom's suggestion, by contouring the barony of Enni-howen in the north of Ireland during the years 1839–40, under the immediate direction of the late Captain Bennett, R.E.

Consequent on this decision of the Government, as the force of assistants who had been employed on the Irish survey completed the work they had in hand, they were successively transferred to England, and employed on the surveys of the counties of Lancaster and York; and after awhile a portion of this force was removed to Scotland, for the surveys of the counties of Wigtown and Kirkcudbright.

The discovery of the precipitation of metallic particles by the voltaic circuit about the same time, by M. Jacobi of St. Petersburgh, and by Mr. Spencer of Liverpool, was no sooner made known than experiments were instituted, under the personal superintendence of Major Larcom, for making this important discovery available for the purposes of the Ordnance Survey; and only a very short interval elapsed before these experiments were brought to a successful termination: so that when the engraving of a copper-plate of any sheet of the map had been completed, of which a large number of impressions were likely to be required to meet the wants of the public, a *duplicate* was prepared, by first electrotyping, as the art is now generally called, a *matrix* of the original plate, corresponding in fact with an ordinary impression on paper, but taken *in copper* instead of *on paper*, and made thicker than the paper, and then from this *matrix* electrotyping a second sheet of copper, which will necessarily be a fac-simile of the original plate, and can be substituted for it in printing off the impressions, the original plate being thus preserved intact: in this manner a duplicate of a plate, which might have cost hundreds of pounds to engrave, could be made, at a trifling addition to the first cost of the prepared copper-plate on which the engraving had been executed. This art is now extensively used in the revision of old and insertion of new work on copper-plates, and in making several duplicates of the same engraved plate, on which different kinds of additional specific information can be afterwards engraved; thus the geological lines and characters are now only engraved on duplicates of the original plates of the Ordnance Maps, as the insertion of so many additional lines as are required by the geologist would tend to confuse and disfigure a map only required for the ordinary topographical purposes.

Up to the close of the Irish survey Colonel Colby had trodden in the footsteps of his predecessor, in adopting the low water of ordinary spring-tides as the datum of altitude for the heights inserted on the Ordnance Maps, and the importance of correctly ascertaining such low-water level, for all purposes connected with the drainage of cities and lands but little elevated above the sea, can scarcely be too highly estimated; but the propriety of the low-water datum having been questioned, on the ground of its variability

dependent on position, and the levelling between Axminster on the English and Portishead on the Bristol Channels, made in 1837–8, under the direction of Professor Whewell, at the charge of the British Association for the Advancement of Science, having proved the disagreement between the low and high waters and the very close agreement of the mean-tide levels at these two places, Colonel Colby, in the spring of 1842, after consulting with and obtaining the advice and suggestions of the Astronomer Royal on the subject, took the necessary measures for ascertaining the variations that exist in the height of the high, low, and mean water of the sea on the coasts of Ireland, and for making the operations subservient to improvements in the theory of the tides, by causing a complete series of tidal observations, extending over a period of about two months, to be simultaneously made at twenty-two different stations round Ireland, and by connecting together the zeros of the tide-gauges at these stations by a series of double levellings, so as to reduce all the observations to the same hydrostatic level. The whole of these tidal observations, rather exceeding 200,000 in number, together with the information respecting the several series of levellings, were placed in the hands of the Astronomer Royal towards the end of the year 1842, who, in his valuable paper in Part I. of the *Philosophical Transactions* for 1845, "On the Laws of the Tides on the Coasts of Ireland," founded on these observations, speaks of them in the following manner:—"The circumstances of place, simultaneity, extent of plan, and conformity of plan, appear to give them extraordinary value; and extent of time alone appears wanting to render them the most important series of tide observations that has ever been made."

From the information so gained, Colonel Colby determined that the heights to be inserted in the Ordnance Maps of Lancashire, Yorkshire, and Scotland, and of the towns drawn and engraved on the respective scales of 6 and 60 inches to a mile, should have reference to the mean level of the sea; and accordingly some tidal observations were made at Liverpool, which enabled an approximate mean-tide level to be assumed for the reduction and working out of the altitudes derived from the various lines of levelling branching from that port: and there is every reason to suppose, from comparisons with the height of mean-water at other ports, that the datum of altitude so settled will be found free from any sensible error.

In 1840, Colonel Colby obtained the loan from the Royal Society, of their large 3-feet theodolite constructed by Ramsden, originally a fac-simile of the one belonging to the Ordnance, but which was greatly improved by Captain Kater during the year 1821, by the addition of four vertical micrometer microscopes for reading the horizontal limb; and Colonel Colby further improved this instrument for the observation of angles of elevation and depression, by placing its telescope between two 12-inch vertical circles, read by two horizontal micrometer microscopes attached to the Y supports, and placed opposite to each other, instead of the

single semicircular arc read by one micrometer microscope, with which it was formerly supplied: and with this instrument, and others belonging to the department which became available about this period, he steadily set to work to revise and correct the errors, as well as to supply the many omissions, in the principal triangulation of South Britain, as carried on under the orders of General Mudge, and to extend it over the eastern counties of England, where no triangulation had previously existed on which any dependence could be placed. These errors and omissions had resulted from the hurried manner in which the work was performed, from the very imperfect means placed at General Mudge's disposal, and from the want of a legislative enactment for the preservation of the various trigonometrical observing stations throughout the country, which want sometimes led to a failure of identity between the observing and observed points; so that, all things taken into consideration, it is rather to be wondered at that the work should, generally speaking, be so good as it is known to be.

From the date of the resumption of the principal triangulation of Scotland in 1838, the charge of the several instruments in the field had been placed under Major (then Lieut.) Robinson, R.E.; but that officer quitted the survey in 1843, to assist in the demarcation of the boundaries between the British and United States dominions in North America under Lord Ashburton's Treaty, and the whole of the observers were shortly afterwards brought under the officer in charge of the calculations at the head-quarters of the Survey; and as Colonel Colby had in 1840 sanctioned an experiment for the training and employment of non-commissioned officers of Royal Sappers and Miners, as observers with the large instruments, and for the adoption of a more refined and systematic method of observing, which experiment proved very successful, the selection of good geodesical observers (for which very strong sight probably ranks as the first qualification, and skilful manipulation as the second) could now be made from a larger number of persons than when it was confined to the officers of Engineers employed on the survey, and the cost of the observing was thus very greatly diminished.

Colonel Colby also made another important advance in the execution of Topographical Maps. Knowing the quality of the principal triangulation of the United Kingdom, and the degree of dependence to be placed on it, and feeling assured that the general figure of the earth was sufficiently well known to admit of the accurate calculation and introduction of the latitudes and longitudes on the margins of the sheets (the trigonometrical points within being all laid down by rectangular co-ordinates), without any risk of material error or chance of misleading the public, he sanctioned their insertion on the first sheet of the 6-inch map of Lancashire, which was published in 1846, not to degrees and minutes as it is usually done, but to single seconds of latitude and longitude; which specific information has been continued on all the sheets since published, and which, united to the representation of the form of the ground by contour lines, renders this map, embracing as it does every detail that appears on

the surface of the ground, by far the most perfect of any yet published.

It will readily be understood, that in carrying on the Ordnance Surveys and in making advances and improvements which have been successively introduced into geodesical operations in this country, Colonel Colby had the benefit of most able and zealous assistance from some of the officers of Engineers employed under his orders, as the names of Drummond, Larcom, and many others sufficiently testify; but although the merit of many important improvements relating to the survey may be individually due to the officers who proposed them (and no one was more ready to allow this than Colonel Colby) yet the credit of sanctioning and causing them to be adopted rests entirely with the Superintendent. It appears tolerably evident that Colonel Colby always contemplated continuing the practice of his predecessors with respect to publications relating to the scientific results of the survey, by employing an officer to take especial charge of that particular portion of the work: thus Captain Drummond had to prepare an account of the measurement of the Lough Foyle Base, a duty which devolved on Lieut. Murphy when Captain Drummond was transferred to the political branch of the Government. Lieut. Murphy's labours on this subject were in turn interrupted by his joining Col. Chesney's expedition down the Euphrates, in the year 1835. At a subsequent period, Col. Colby appears to have contemplated completing the work himself, but the occupations connected with the general superintendence of the Ordnance Surveys prevented him from giving his personal time and attention to it, and the preparation of the account of this operation, as well as that of the publication in one volume of the whole of the observations made on the survey with Ramsden's zenith sector, between the years 1802 and 1836, including those made at Balta and Dunkirk in 1817–18, in conjunction with MM. Biot and Arago on the part of the Institute of France, was entrusted to Capt. Yolland, R.E.

It is also understood, that when General Colby quitted the survey in 1847, in consequence of his promotion, the printing of the observations made with Airy's zenith sector for the determination of the latitudes of nineteen stations connected with the several ares of meridian in the United Kingdom, and exhibiting the effects of local disturbance of the plumb-line at various places to a sensible amount, was in hand: and by the report of the proceedings at the last meeting of the British Association the work appears to have been completed, although it has not yet reached the Society. Col. Colby was endowed with great administrative qualities. Possessed of a very sound judgment and great energy of character, and conscientiously alive to the consequences of failing to carry out the great work with which he had been entrusted in a manner creditable to the country and to the department to which he belonged, he never hesitated to put before the authorities under whom he served his views as to what should or should not be done, and the reasons for it: and these views were usually expressed in very clear and

explicit terms. He allowed no selfish considerations to interfere with the direct line of his duty: for instance, having obtained power from the Master-general of the Ordnance to retain only such officers of Engineers as he found well adapted for carrying on the peculiar duty on which they were engaged, he did not scruple to ask for such officers as he considered most likely to advance the work he had in hand, and to return to the ordinary duties of the corps those whose pursuits or talents were not likely to further the progress of the work.

As an instance of his energy and self-reliance, it may be seen by reference to the returns connected with the Irish Survey presented to Parliament, that after he had fully succeeded, in 1835, in getting his system into perfect working order, and when finished plans of the country were annually completed, to the extent of between two and three millions of acres, he did not hesitate, year after year, to take upon himself the responsibility of exceeding, by large sums, the votes sanctioned by Parliament, rather than to diminish the rate of expenditure and progress by discharging efficient assistants, which reduction would necessarily have had the effect of deferring the final completion of the work for a longer period of time. On the completion of the plans of the Irish Survey, he endeavoured to procure for a few of the officers of Engineers employed under him, whose services he considered to be fully entitled to it, the ordinary mark of military approbation, viz. a step of rank by brevet promotion, that their exertions might thus be recognised in the corps to which they belonged; but he was unsuccessful in his efforts, although the then Master-general of the Ordnance (the late Lord Vivian) fully admitted those services, and apparently only deferred compliance with the recommendation till the engraving should be complete, coupling the recognition of those services with the more pre-eminent ones of the chief under whom they served; and it certainly is to be regretted that this decision should have been permitted by the Ordnance department to be final, at all events as regards Colonel Colby himself, as it is certain that no work executed by the Ordnance during the present century has reflected so much credit on the department as the successful completion of the Ordnance Survey of Ireland, which, in addition to completely serving the purpose for which it was undertaken, has already been used for the following public purposes:—It has formed the basis of the Poor-law Boundaries in detail, determining the localities called Electoral Divisions, according to which the Poor-law Assessment is made; it has served for the Poor-law Valuation, which includes tenements, and which is distinct from the Townland or General Valuation, of all Ireland; it has been used as a means of obtaining an accurate annual return, at very small expense, of every variety of agricultural produce in Ireland; it has been made the basis for the Irish Census, and employed in the sale of property, the boundaries of the fields and farms sold being laid down on the map, and accurately coloured, each map being subsequently enrolled, so that at any future time the property sold can

be traced on the map, and from it identified on the ground; it has further been employed in carrying out the provisions of the Land Improvement Act; and last, but not least, it has greatly facilitated the task of preparing and carrying on, in the most economical manner, the various engineering works executed in that country.

General Colby was an original Member of the Astronomical Society, and one of the Committee appointed to frame rules and regulations for its government, at its formation in 1820. He was also appointed a Member of the Board of Longitude in 1822, and continued to act in that capacity until the Board was dissolved, by Act of Parliament, in 1828. He was also a Fellow of the Royal Societies of London and Edinburgh; of the Geological and Geographical Societies of London; a Member of the Royal Irish Academy, and of the Geological Society of Dublin; an LL.D. of Aberdeen; and a Knight of Denmark.

Major Colby was promoted to Lieut.-Colonel in July, 1825; to Colonel in January, 1837; and to Major-General in November, 1846. He married in 1828, Eliza H., second daughter of Archibald Boyd, Esq., of Londonderry, and died on the 9th October, 1852, at Liverpool, in the 69th year of his age, leaving his wife and seven children (four boys and three girls) to lament the irreparable loss they have sustained.

Samuel Elsworth Cottam was born on the 1st of June, 1801. He left school at twelve years of age, from which period his education was entirely the result of his own efforts. Apprenticed to a large mercantile firm in Manchester, till they retired from business, he continued in the service of the trustees, as agent and confidential servant, till his decease, having been thirty-six years in their service. He became a member of the Manchester Mechanics' Institution in 1825, and was shortly afterwards elected its honorary secretary. He obtained considerable reputation as a popular lecturer, within its walls, on chemistry and electricity; and also delivered numerous lectures on these and other branches of natural philosophy in various parts of the country. He was actively instrumental in establishing the periodical exhibitions of objects of industry, art, and science, which have now grown to such gigantic proportions both in this and the sister kingdom, and which originated with the Manchester Mechanics' Institution. Mr. Cottam had for some time the care of the classes for arithmetic, algebra, and geometry, and his qualifications as a teacher of these branches of knowledge were entirely self-acquired. In 1838 he became Secretary to the Manchester Society of Guardians for the Protection of Trade, and retained this appointment till his death. For a time he was one of the Honorary Curators of the Natural History Society; and he was a member of numerous societies for the promotion of science, not only in Manchester, but also in the metropolis. Amongst others, he took great interest in the scientific *soirées* of the Victoria Gallery, held in the suite of rooms attached to the Manchester Exchange; and when its collection of scientific objects

and apparatus was disposed of—being unwilling to see sacrificed under the auctioneer's hammer the valuable plate electrical machine, and the rest of the electrical apparatus (all of which had been prepared under the direction of that able electrician, the late Mr. Sturgeon, director of the Victoria Gallery)—Mr. Cottam became the purchaser of that apparatus; and one of the latest acts of his useful life was to present this collection to the trustees of the Owen's College. Of late years he had been largely engaged in his profession as a public accountant, in auditing the accounts of various railway companies; and some of his friends trace the foundation of the illness which carried him off, to the great mental labour and unremitting toil imposed upon him in connexion with a large railway company in the North of England. Mr. Cottam died August 7, 1852, leaving behind him a high character for moral worth and scientific utility.

George Dollond, F.R.S., &c., was born in London on the 25th of January, 1774, and having the misfortune to lose his father when very young, his education and subsequent management devolved upon his maternal uncle, Peter Dollond, who was then carrying on the business in St. Paul's Churchyard, to which the nephew ultimately succeeded, (on which he took the name of Dollond, his father's name having been Huggins). Accordingly, he was placed at an early age at the seminary conducted by the late Mr. George Lloyd, at Kennington, then celebrated for the able manner in which his pupils were prepared for their different positions in the world. He was not, however, long allowed the benefit of his instruction, for early in the year 1787 he was sent to the manufactory of Mr. Fairbone, to learn the trade of a mathematical-instrument maker, and in March, 1788, commenced an apprenticeship to the business for which his family were so famed.

There is scarcely anything found in his early years worthy of record, with the exception of the great assiduity with which he applied himself to obtain a mastery of his chosen profession, and the narrow escape he had in 1792 of closing his career from a severe attack of illness, which threatened for a long time to have a fatal termination. Recovered from his illness, and having passed safely through the remainder of his apprenticeship, we next find him on his own account assisting the business of his uncles with his talents and labour, and for a considerable time the whole of the mathematical department of that establishment was the production of his manufactory. The death of his uncle John Dollond, in 1804, a younger brother and partner of Peter Dollond, caused a change in his affairs, for it being found necessary to fill up the gap occasioned by that untimely event, he was immediately selected by his uncle for the post, and in November, 1805, he assumed the position he was fated so long and so honourably to fulfil, both in conjunction with his uncle, until the latter's retirement in 1819, and afterwards on his own account, until the termination of his career. It is unne-

cessary to enumerate the many instruments by which he maintained the celebrity of his establishment, but the Atsmospheric Recorder deserves some notice, as being the closing effort of his long life, and as having obtained for it the reward of the Council Medal of the Great Exhibition of 1851. He was elected a Fellow of the Royal Society on December 23, 1819, and contributed various papers, which were published in the *Philosophical Transactions*. But it is by his connexion with the Royal Astronomical Society that George Dollond will be mostly recognised, for in the course of his long life he was honoured with the friendship of many of the most illustrious representatives of science; and in one of the social gatherings arising out of these intimate relations the formation of the Astronomical Society was proposed. These few lovers of astronomical science formed the nucleus of this body, and it was by their instrumentality and exertions that a Royal Charter was obtained for it, and it was raised to the eminent position which we see it maintaining at the present time. Few of his fellow-labourers in that undertaking survive him; many were removed before the valuable results of their efforts could be known: but he to the last was most indefatigable in the interest he took in the advancement of the Society, and was a constant attendant at its meetings or its council table, until declining strength warned him that the all-powerful mind must at length become subservient to the failings of the flesh, and that a more secluded field of action must for the future be his portion. He lived but a short time in retirement, for the feebleness that had compelled him to resign the management of public business gradually increased upon him, until on the 13th of May, 1852, at the age of seventy-eight, he passed away with scarcely a struggle from a circle he had adorned, leaving behind to posterity an unsullied reputation and an honoured name.

James Edmonstone was born in Edinburgh, February 17, 1802. In early life he had a strong wish for a naval life, which was opposed by his father. This disposition, joined to feeble health, appears to have prevented his settling himself to any occupation, and he passed his youth in the acquisition of knowledge, without any determined purpose. In 1838 he took orders in the Church of England, and proceeded to Sydney, where he exercised his function till the end of 1846, when he returned to England. After doing duty for some time at Romney, he took up his residence at Twickenham, where he formed a large and valuable collection of books, and where he died, March 19, 1852. He was a man of knowledge and energy, and retained his passion for nautical matters to the end of his life. He was an instance of a man whose talents were prevented from acting in the direction to which a strong vocation called them, by what the friends who have furnished materials for this notice call the "great cross and trial of his life" — the refusal of his father to allow of his entering the navy. He was attached to astronomy, as the parent science of

navigation, and to all the branches of knowledge which interest the seaman or the traveller.

THOMAS JONES was born on the 24th of June, 1775. In the year 1789 he entered Mr. Ramsden's establishment in Piccadilly, where he acquired a knowledge of the art of constructing and dividing astronomical instruments. Subsequently he commenced business on his own account, and soon attained a high reputation among astronomers for his skill in the construction of instruments of the larger class used in observatories. He assisted, in conjunction with Dr. Pearson, Mr. Troughton, Captain W. H. Smyth, R.N. and others, in the formation of the Astronomical Society. Afterwards he was elected a Fellow of the Royal Society. He was also a member of the Royal Geographical Society, a member of the Institution of Civil Engineers, and a member of the Statistical Society of France. He died on the 29th of July, 1852.

The following astronomical instruments were constructed by Mr. Jones in the course of his labours as an artist:—

A 5-feet equatoreal, Cambridge Observatory.
A 6-feet mural circle, Royal Observatory, Greenwich.
A 6-feet ditto ditto Cape of Good Hope.
A 5-feet ditto Armagh Observatory.
A 4-feet transit circle, Paramatta.
A 6-feet ditto Radcliffe Observatory, Oxford.
A 10-feet transit instrument, A 6-feet mural circle, A 5-feet equatoreal, } Royal Observatory, San Fernando, Cadiz.
A 6-feet mural circle, Trevandrum.

JAMES LOCKHART (whose decease was mentioned in the last Annual Report) was born on the 26th of July, 1763, in the city of London. His father was James Lockhart, of Shorfield House, near Rumsey, Hants, formerly a banker at the west end of London. He was a descendant of the old Scotch family of that name, and on the female side from the sister of Oliver Cromwell. He married Miss Gray, a member of the Society of Friends, by whom he had three children.

James Lockhart, the subject of the present memoir, was the eldest child, and received his early education at Reading School, whence some time after he was removed to Eton College, and was there at the same time with the late Duke of Wellington, who, he said, was always remarkable, when studying there, for his taciturnity and grave disposition.

James Lockhart was, from his earliest youth, fond of study, and when he left Eton was considered a very proficient scholar in Latin and Greek, and noted for his Latin versification, a fondness for which he retained throughout his life. On leaving Eton he entered his father's bank, in which he subsequently became a partner. While here, he originated the Cattle Insurance Company. Being of a cheerful and communicative disposition, and possessed

early of ample means, he entered much into society. On retiring from the banking business in 1799, he settled with his family amidst the romantic scenery of the lake district on the banks of Windermere, after losing his wife. This lady was the daughter of Mr. Coxe of Philadelphia, a very extensive owner of land in America, which, however, was wholly forfeited during the American revolution, owing to Mr. Coxe's siding with the royalists. By this wife he had three daughters and one son.

Amidst the repose of this charming scenery he began more zealously to study mathematics, and kept up a constant correspondence with Francis Maseres, Bonnycastle, Peter Barlow, the late William Frend, and Dr. Isaac Milner, President of Queen's College, Cambridge. There he also formed the acquaintance of Dr. Watson, the learned Bishop of Llandaff. About 1801 there appeared a singular work, which much amused the reading world, entitled *Dr. Syntax*. It was Mr. Lockhart who suggested the idea of this work to its author, Mr. Coomb.

In the year 1805 he married his second wife. By this lady, who was much younger than himself, he had eight children,—six sons and two daughters. In 1819 he left the north of England for Holland, to give his numerous family the benefit of a more general education. After passing some time at Leyden, he purchased a comfortable house and large garden in the pleasant suburbs of the ancient town of Haarlem. Here he devoted, with untiring patience, the greater portion of his time to the education of his younger children, in which a knowledge of arithmetic ever formed the basis. Any one, however old, young, poor, or rich, who was fond of mathematics, immediately won his friendship; and many were the young persons to whom he gave mathematical instruction, not for any pecuniary remuneration, but for the mere love of the science he so much admired. In this matter his zeal was unbounded; and many there are still living, here as well as abroad, who have to thank him as the founder of their prosperity. Among his earliest pupils in this way we may mention Dr. Joshua King, now President of Queen's College, Cambridge. This boy he discovered to be a genius, and under his private tuition, in his leisure hours, made considerable proficiency in French and mathematics. Mr. Lockhart was instrumental in sending this young man to Queen's College, Cambridge, with a letter of recommendation to his friend Dr. Isaac Milner, then President of Queen's College, remarking, "I *herewith send you a northern star.*" Mr. Lockhart's method of communicating ideas was particularly pleasant and winning,—indeed, he only considered it his duty as a member of the human family to impart knowledge to others, and continually to instil into the minds of youth grander conceptions of an Infinite Being. He always conveyed to their minds the idea of a *Deus geometrisans* pervading the whole of creation. While in Holland, one of his chief amusements was the cultivation of flowers, particularly of the dahlia. This beautiful flower was then only just becoming known, and had not long been brought from America. In the space of six

or seven years, from one solitary, single-flowered dahlia, he had raised the most choice collection to be found in that country, or perhaps in the world. Persons travelled from a distance to see them. He attended to them himself alone.

As he was possessed of the means of living comfortably and independently, his house was ever open to the scientific; and here, as well as in England, he was accustomed to offer for solution certain algebraical problems to the public, always attaching to each a certain prize in money or books. Among Mr. Lockhart's numerous acquaintance while staying in Holland, we may mention the ex-king of Sweden, Gustavus Adolphus, who was very partial to his company, himself an author, and very fond of astronomy. This unfortunate ex-king used to relate his adventures with all the enthusiasm of a Charles the Twelfth, to whom it was his wish to be compared; possessing, however, but one of his characteristics,—obstinacy!

After staying many years in Holland, he returned to England with his family in 1835 upon the death of his brother, John Ingram Lockhart, who died without issue, and was Recorder of the city of Oxford, and for many years a representative of that place in Parliament. He took up his abode near Aylesbury, whence, in 1837, he removed to Cowley Rectory, near Oxford, more particularly with the view of watching over his late brother's affairs, whose estates had unfortunately got into Chancery. Notwithstanding this harassing suit, his mind ever remained serene, and he seemed to forget all about it on turning to his never-absent companions, the slate and pencil. In 1841, on the sale of the Cowley estate, he removed to Brompton. It was here, in the year 1843, that he had the great misfortune to lose his second wife.

In the year 1845 he took up his abode in the Isle of Skye, among the distant Hebrides, where he took a shooting district comprising the adjoining island of Razay, belonging to the ancient family of Macleod of Macleod. After a sojourn of a couple of years in this island, universally esteemed, he removed into Argyllshire, and took one of the old mansions of the Stewarts of Appin, situated amidst the most beautiful and romantic scenery imaginable. He was so great a favourite with the country people that they gave him the name of Fingal. After the term for which he had taken Fashnacloich had expired, he removed in 1851 to the other extremity of Great Britain, into Somerset, where he resided at Clitsome House, near Washford. Here his usual good health began to decline, and he would remark that he felt at times he was growing old; yet he would frequently mount his horse and ride to the top of some eminence, attended by his Highland servant, for the purpose of indulging in his favourite sport. Only a few days before his death he shot a pheasant, without the use of spectacles, at 63 yards' distance. He died at this house on the 6th of January, 1852, in the 89th year of his age, in full possession of his faculties. There is no doubt that a harassing and expensive chancery suit, with the greater number of his children dependent

on him, hastened his death at last. His last moments were perfectly composed, and he departed with the words, "God has shown me many mercies. I am not afraid to die." He was never ill in his life, and used to say he never felt thirsty. He was a remarkably early riser; and said that most people killed themselves by eating and drinking too much. He left twelve married children behind him, one of whom is also a Fellow of this Society. According to his own request he was borne to the grave by twelve poor men, and lies buried in the churchyard adjoining Nettlecombe Court, the seat of Sir Walter Trevelyan. There the scenery is strikingly picturesque, surrounded by woods and gentle slopes, he having himself made choice of this lovely spot.

He was a man whose knowledge extended over a variety of subjects, ever cheerful in society, with a most winning mode of arguing, deliberate without being cold, mathematical without being dry. In the field of argumentation he often used to say that he had learnt much from the celebrated John Stewart, commonly called "the Walking Stewart," author of the *Opus Maximum*, whose memory he greatly esteemed. Mr. Lockhart's knowledge of languages was also considerable, comprising Greek, Latin, German, Welsh, Dutch, French, Italian, and latterly he studied the ancient Celtic. Of music he was also passionately fond, and played the violin, flute, and flageolet with great taste.

Mr. Lockhart was one instance of the remarkable class who are led by strong predilection to the cultivation of one particular branch of mathematics, to which they devote a part of the leisure of their whole lives. His subject was the numerical solution of equations. From 1813 to 1850 he published various works on this subject, in English and Dutch; and it appears by his correspondence with Maseres that he was a diligent investigator of its history as well as theory. The methods of Horner and Sturm did not come into prominent notice till he was more than seventy-five years of age, yet he made himself master of them and wrote on them. It has been said that the taste for mathematics declines with increasing years. If this be true, the instances to the contrary become the more remarkable; and perhaps no one on record is more striking than that of the cultivator of a branch of algebra who acquired new knowledge of it at seventy-five, and published on it at eighty-seven.

Works published by James Lockhart:—

1. *A Method of approximating towards the Roots of Cubic Equations belonging to the irreducible case.* 1813.

2. *Nieuwe en Algemeene leerwyze, diquadraten op to lossen, waarby de Systemas van Descartes en Euler tot biquadraten met derzelven tweede termen worden voortgezet.* Haarlem, Holland, 1833.

3. *Nieuwe oplossing van Cubiek vergelykingen door juiste uitdrukkingen ook by nadering zonder, beproeving af gissing.* Haarlem, 1825.

4. *Resolution of Equations.* Cowley Rectory, Oxford, 1837.

5. *Resolution of Two Equations.* Oxford, 1839.

6. *Extension of the celebrated Theorem of C. Sturm, whereby the Roots of Numeral Equations may be separated from each other, with copious examples.* Oxford, 1839.

7. *Resolution of Equations by means of Inferior and Superior Limits.* Inscribed by permission to the President and Council of the Royal Astronomical Society. London, 1842.

8. *The nature of the Roots of Numerical Equations.* "*Longum iter est per præcepta, breve et efficax per exempla.*"—*Seneca.* Fashnacloich, Argyllshire, May 1850. Published in London.

Mr. Lockhart says, in his preface to this work:—"Following the example of Dr. Prideaux, we must beg the reader to excuse, on account of our age, now verging towards 87 years, any errors he may find in this treatise, either of computation or doctrine: no one has assisted in either."

Thomas Norris, Esq., of Howick House, near Preston, Lancashire, died on the 15th of March, 1852, aged eighty-seven. He was born at Croston in the same county, and having received a good provincial education, entered at the age of twenty, as book-keeper, into the eminent firm of Peel, Yates, and Co., cotton-spinners and calico-printers. Having by his industry and business habits obtained a confidential position in the house, he subsequently became a partner. In 1821, Mr. Norris retired from the business, in possession of a large fortune. During the leisure afforded him from business Mr. Norris cultivated, by such means as were at his command, a liberal and elegant taste for science and the fine arts, and on his retirement into private life he collected, with great discrimination and zeal, pictures, coins, minerals, shells, insects, and other objects of natural history. He also indulged his taste in astronomical observations, within a few years of his death; he was in the daily habit of using the transit instrument, equatoreal, &c.; he ground and polished glass and mirrors, and constructed microscopes and telescopes. His judgment in matters of art is sufficiently attested by the large and valuable collection which he formed of pictures of the old masters, and he possessed a vivid discrimination of the specific character of objects of natural history. His collections of insects and shells were of considerable value. Mr. Norris was a gentleman of very retiring habits, but exceedingly hospitable and kind to those of kindred pursuits, who were desirous of profiting by the use of his collections. He was extremely prompt and liberal in his dealings; hence, when any rarity in coins, shells, insects, found its way into the market, he was generally the first to whom it was submitted for sale. In his periodical visits to London, he seldom failed to call in Whitehall Gardens to see Sir Robert Peel, the renowned son of his friend and partner in trade, and the great statesman received his visits with peculiar gratification and interest.

The Rev. Charles Turnor, M.A., F.R.S., was born in Lin-

colnshire on the 10th of August, 1768. He was many years vicar of Wendover in Buckinghamshire. In the year 1818 he was collated to the prebendal stall of Sutton in the Marsh, in the cathedral church of Lincoln. Mr. Turnor was descended from a family of that name of long standing, of Stoke Rochford, and Penton House, in the county of Lincoln. Among his ancestors were Sir Christopher Turnor, one of the Barons of Exchequer, in the year 1660, whose portrait is in Guildhall, as one of the judges appointed to assess the damages occasioned by the great fire of London; and his brother, Sir Edmund Turnor, paymaster to the forces in the reign of Charles I., who was taken prisoner at the battle of Worcester in 1651.

The property of Sir Isaac Newton, about three miles from Stoke Rochford, was purchased by the family about four years after Newton's death, and its integrity remains much the same as in Sir Isaac's time. This led Mr. Charler Turnor during a series of years to make a valuable collection of all he could find connected with the illustrious philosopher.

Mr. Turnor died on the 12th of January, 1853. In private life he was beloved and respected by all who had the pleasure of his acquaintance. Besides being a liberal patron of science, he was a clever artist and an excellent connoisseur of all objects of taste and *vertù*.

The interest which Mr. Turnor felt in the progress of science, more especially astronomy, is amply evinced by his valuable donations to various scientific institutions. In 1836 he presented to our Society a work edited by his brother Edmund Turnor, entitled "Collections for the History of the Town and Soke of Grantham," which, among other matters of interest, contained some authentic memorials of Newton, published for the first time from original MSS. in the possession of the Earl of Portsmouth. In 1845 he also presented two MS. volumes of Astronomical Tables executed in the fourteenth century. An account of these curious manuscripts, drawn up by Mr. Harris, the late assistant-secretary of the Society, is inserted in volume xv. of the Memoirs. Among the benefactions of Mr. Turnor to the Society may be also mentioned the marble bust of Newton, which adorns the meeting-room.

In 1846 Mr. Turnor presented to the Royal Observatory at Greenwich an astronomical clock with gridiron pendulum, by Mudge and Dutton, which was used for some time in connexion with Troughton's mural circle, and which is now in constant use in connexion with the reflex zenith tube of that establishment.

The Council have to announce that Mr. Turnor has bequeathed to the Society the sum of 500*l.* three per cent consols, to be laid out in books for the library. As the news of this legacy is very recent, the Council are not prepared to state the precise mode in which they will proceed to carry into effect the testator's intention. In all probability it will be advisable to form a fund, which will, of course, bear Mr. Turnor's name, the interest of which will be regularly laid out in increasing the library, and, it may be, part of

the principal from time to time, as advantageous opportunities shall occur. By the liberality of authors and institutions, the Society receives almost all new books of astronomy as fast as they appear; and those who know the market of older books are aware that it would be almost impossible to spend 500*l.* to real advantage in a very short time. The perfectly open manner in which this legacy is left, as to mode of proceeding, is, and should be felt as, an additional benefit.

It is understood that Mr. Turnor has also made a bequest to the Royal Society, which includes some interesting memorials of the illustrious Newton.

On the Great Transit Circle of the Royal Observatory of Greenwich we have no remarks to make which would not be a repetition of those made in the Council Report of last year. But it may be interesting to the Society to be informed that an instrument of exactly the same dimensions (differing only by a small quantity in the focal length of the object-glass), and of the same form and materials, is now nearly completed, for the observatory of the Cape of Good Hope. It will be recollected by those members of the Society who are well acquainted with the Greenwich instrument, that Mr. Simms pointed out, after the completion of the instrument, that great facility would be given for the adjustment of the collimators by perforating the central cube. This improvement has been adopted in the Cape transit circle: in other respects it is similar to the Greenwich transit circle.

The Reflex Zenith Tube of the Royal Observatory is now brought into an efficient state. An accident caused an interruption of observations in the spring of last year, and, therefore, the whole number of observations hitherto obtained is insufficient to give means for a definitive judgment on its merits. Their results, however, appear to be exceedingly accurate. It appears (as was thought probable from the first) that the loss of light in the successive transmissions and reflexions diminishes sensibly the brightness of the image, and, consequently, the number of observations which can be secured in a given time. This difficulty may probably be removed by the use of a larger object-glass; meantime it may be observed, that the instrument appears to possess that freedom from the liability to errors, either constant or accidental, which it was Mr. Airy's principal object to obtain.

The use of Galvanism, as an ordinary agent in the daily business of the Royal Observatory, is now almost completely established. One clock is adjusted every day, by a peculiar mechanical arrangement, to accurate Greenwich mean time. By means of this clock, with the sympathetic galvanic construction arranged by Mr. Shepherd of Leadenhall Street, four other clocks in the Observatory (one of them having a large external dial for the use of the public) are kept going to accurate Greenwich mean time. By means of the

same clock, hourly signals are sent to London for use on the railways, the Greenwich time-ball is dropped, and the time-ball erected by the Electric Telegraph Company, on their office in the Strand, is dropped. Within a short time experiments have been made for verifying the apparatus which it is intended to employ for the transmission of longitude-signals to distant observatories.

The American method of observing transits has not yet been brought into action at Greenwich. Although the principal parts of the apparatus are prepared, there are yet some petty defects which have prevented its practical application. The great demand, at the present time, for workmen who are skilled in the constructions required for galvanic telegraphs, causes great delay in the preparation or correction of galvanic apparatus.

Since the last Report, the eleventh volume of the *Radcliffe Observations* has been published, which contains a description of the heliometer, accompanied by an engraving. Mr. Johnson has been enabled during the past year to devote his entire attention to this instrument, and the result has been an extensive series of observations of 1830 *Groombridge*, and 61 *Cygni*, in reference to the question of parallax.

The reductions for the circumpolar catalogue are advancing satisfactorily. It is expected that that work will be ready for press in the course of the present year.

It may be interesting to the Society to be informed, that the Radcliffe Trustees have given directions for laying down a line connecting their Observatory with the Electric Telegraph of the Great Western Railway, and through that medium with the Royal Observatory. That the communication is not already completed is entirely owing to the circumstance of the ground being flooded through which the wire will pass. Mr. Johnson expresses himself much indebted to the Directors of the Electric Telegraph Company for the cordiality and liberal spirit in which they have entertained the project.

The course of observations carried on at Cambridge in the present year is a continuation of that commenced at the beginning of 1850, consisting of meridian observations of the sun, the moon, and moon-culminating stars, the recently-discovered planets and zodiacal stars, and equatoreal observations of the recently-discovered planets and comets.

At the commencement of the current year Prof. Challis made an alteration in the scheme, which had become absolutely necessary on account of the accumulation of unreduced observations. He proposes to confine the meridian observations to zodiacal stars, of which he is gradually forming a catalogue, which, when complete, will include the majority of stars of the 9th magnitude, and brighter, contained in a belt extending 5° on each side of the ecliptic. Meridian observations of the small planets are attended

to at Greenwich, and with means more powerful than those of the Cambridge Observatory. Prof. Challis intends, however, to pursue these bodies with the Northumberland telescope, especially when they are near quadratures, or have become very faint; and the observations will thus be, for the most part, supplementary to the meridian observations at Greenwich. The large telescope will also be employed on classes of observations which entail no large amount of calculation, such as measures of double stars, mapping portions of the moon's surface, &c. By persisting in this plan, Prof. Challis hopes, with the aid of an additional computer, which the University has allowed him, by degrees to bring up the arrear of unpublished observations.

At the Edinburgh Observatory the labours of Prof. Piazzi Smyth begin to assume a more independent character than they have hitherto done. In the course of the last year the observations of 1844-5-6-7, reduced in the usual manner, have been printed and published in one volume, the tenth of the Edinburgh series; thus including the last of the work of the late lamented Prof. Henderson, and accomplishing the end which his successor thought it a pre-eminent duty to strive to obtain.

The subsequent observations of 1849-50-51-52, are far advanced in computation, and some of them ready for the printer whenever the necessary leave to go to press shall have been obtained. Meanwhile observations of stars for proper motion, and of stars observed with comets and small planets, are being proceeded with; and measures to make the Observatory more efficient in instruments and in personal establishment are under the consideration of the Treasury.

At the Observatory of Durham the same course of observation has been generally pursued as in previous years. The principal objects have been the small planets. Mr Carrington held the office of observer until March. His observations of *Victoria*, *Egeria*, *Flora*, and *Encke's Comet*, have appeared in the *Monthly Notices* and in the *Astronomische Nachrichten*.

Mr. William Ellis became observer at the beginning of April, and his observations, which are to be found in the *Monthly Notices* and *Astronomische Nachrichten*, have included *Flora*, *Melpomene*, *Fortuna*, *Neptune*, *Irene*, *Massilia*, *Eunomia*, *Hygeia*, *Calliope*, *Lutetia*, *Thalia*, *Hebe*, and *Westphal's Comet*.

The right ascensions of many of the stars of reference have been determined by meridian observations with the transit circle.

During the last few months the mounting of the Fraunhofer equatoreal has been rendered more stiff; and the observations since taken with it are regarded as considerably more trustworthy in right ascension.

In the spring a series of direct and reflected observations were made of stars near the zenith, to the north and south, for the purpose of verifying the assumed latitude of the Observatory. The

result of the whole does not differ from that previously assumed, by half a second of space. The longitude having been found by chronometrical comparison with Greenwich, made by Mr. Carrington, the geographical position of the Observatory may be considered to be very nearly determined.

Mr. Chevallier has been in communication with several observatories in which particular attention is paid to those planets, and finds great readiness to concur in carrying out any well-digested scheme of observations.

The following points appear to deserve attention :—

1. Preparation of exact ephemerides, and, perhaps, of maps of stars in the path of each planet.

2. Distribution of particular planets to certain observatories, regard being had to the means of the observatory and its geographical position.

Thus, in the coming opposition, *Egeria* will be invisible at every observatory in the north of Europe; and often in the north of England the faint planets can scarcely be observed in the summer nights.

3. Observations, early and late, at each appearance, at observatories possessing powerful equatoreal instruments.

4. Good determination, and publication of the position of reference stars.

In speaking of the Liverpool Observatory, we need only repeat in general terms that it continues to justify the high praise which we have always hitherto had the satisfaction of bestowing upon it. It is especially the observatory where the most accurate observations are made for *immediate* use. A striking instance of this kind of astronomical service was lately acknowledged by Mr. Hind, who was enabled, by Mr. Hartnup's excellent observations, to compute a satisfactory orbit of a new planet almost immediately on its discovery. The astronomical observations, taken with the splendid equatoreal during the past year, have been mainly confined to the small planets revolving between *Mars* and *Jupiter*, Brorsen's two comets, and the periodical comet of Encke. All these observations have been published in the *Astronomische Nachrichten*, and a considerable portion in the *Monthly Notices*. As the number of new planets is now so large, and increasing so rapidly, Mr. Hartnup has been advised to limit his attention to those which may be newly discovered, for the purpose of securing an accurate orbit as early as may be, and to observe those already pretty well known only when they pass the meridian of Greenwich by daylight. It may, perhaps, be found necessary to make a further division of labour, and that the astronomers of Cambridge, Durham, and Liverpool should come to some arrangement for a planetary partition.

It is necessary to keep in mind, that purely astronomical observation is by no means the principal object of the Liverpool Observatory. It was founded for the purpose of improving navigation by testing and rating chronometers. It appears that, by degrees,

the utility of this institution is becoming better known to seamen, and is rapidly fulfilling its object. Mr. Hartnup tests all the chronometers sent for trial in different temperatures, and sends them out *with a table of rates corresponding to the temperatures.* The maker is thus enabled to perfect the compensation, or the seaman can allow for the defect. Mr. Hartnup is of opinion, from the records at the observatory, that when the rate of a chronometer is thus corrected, no *constant* difference can be detected between the shore and sea-rates; and that, consequently, local or magnetic attraction, or the motion of the ship, does not sensibly disturb the performance of a chronometer on board our merchant ships.

The time-ball at the Liverpool Observatory is regularly dropped at 1^h P.M. Greenwich mean time, and is of considerable use to chronometer-makers and raters who have not the means of determining the time themselves. But the situation is not well adapted for the use of the seaman *after he has got his timekeeper on board*, and it is, we understand, in contemplation to drop another ball at the same moment from a more conspicuous station, just as is now done at Charing Cross and Greenwich. It is exceedingly desirable to force upon the seaman's notice these aids to the security of navigation; and it is to be hoped that very shortly few ships will leave the Mersey with a badly-rated chronometer, and none without correct Greenwich time at starting.

The meteorological instruments at the Liverpool Observatory are of a very high class, and the results, carefully tabulated, are regularly furnished to the Registrar-General.

The patrons of the Liverpool Observatory have shown their appreciation of Mr. Hartnup's services in the handsomest manner, by making a large addition to his salary, and, as we understand, without any dissentient opinion.

So many well-meant attempts for encouraging science have proved failures, that we cannot help congratulating the founders and supporters of the Liverpool Observatory on their success, which must have exceeded their anticipations; though, from the liberal and business-like way the matter was undertaken, this success has been richly deserved. We trust it may excite emulation and imitation in other quarters.

In the recent numbers of the *Monthly Notices* our members will have seen the prosperous event of Mr. Lassell's mission to Malta. That a person of moderate means, engaged in business, with a wife and family, should have encountered so troublesome, so laborious and expensive an undertaking, for the pure love of astronomy, is a most gratifying occurrence, and shows that the genuine spirit of chivalry still exists in these very material times. We are happy to learn that Mr. Lassell thinks himself repaid, and that his observations of his own satellite of *Neptune*, of the two fainter satellites of *Uranus*, and of the faint satellites of *Saturn*, are so good and so numerous as to settle on a firmer basis the elements of these minute but interesting bodies. Mr. Lassell is on the point

of returning, and while we may congratulate him upon what he *has* done, we trust that his health and strength may suffice for many years to carry on his observations and discoveries successfully at home, though in a less favourable climate than Malta.

The British Association, at its meeting at Belfast in 1852, appointed a committee to consider the steps which might be taken for the establishment, in the southern hemisphere, of a reflecting telescope of large dimensions, to be employed principally in the examination of southern nebulæ. This committee has communicated with the Royal Society, and a joint committee, under Lord Rosse as president, and including several members of this Society, has once met to discuss the subject. It is unnecessary to say that no definitive resolution has yet been passed; but several questions have been started, the consideration of which may probably lead to the introduction of new instrumental constructions, by which the convenience and the power of the mighty telescopes of modern astronomy may be increased; and to the careful selection of stations in which these telescopes may most advantageously be employed.

With the assistance of Mr. William Simms, jun., Mr. Sheepshanks had completed the comparison of all the bars of Baily's metal with bronze 28, before the last summer vacation.* The results, though satisfactory when each observer was considered by himself, did not agree so well when the observers were compared together; and on his return to England, Mr. Sheepshanks suggested that the four selected Government standards should be *recompared*, and by more observers. The Astronomer Royal approved of the suggestion, and, though much to his inconvenience, was kind enough to offer the services of Messrs. Dunkin and Henderson, who are observers at Greenwich. Mr. W. Simms, sen. offered his assistance, which was gladly accepted; and Mr. Warren De La Rue also brought his nice hand and practised eye to the task. Mr. William Simms, jun., as heretofore, adjusted the bars for observation,† and made observations. A considerable number of comparisons of the selected bars with bronze 28 were thus made by each observer, and with the bars in exchanged positions. These observations have just been reduced by the Astronomer Royal, and the results arrived at may be considered, on the whole, as tolerably satisfactory, though short of what Mr. Sheepshanks had confidently expected. The probable error of a *comparison* for each observer is found to be little more than twice the probable error of a *bisection*, as it ought to be; thus showing that the effects of indirect lights, of temperature, of change in the apparatus, &c., are fairly eliminated in the course of the observations. The pro-

* Of the bars thus compared some were objectionable from faults in the division or surface, or as not floating even.

† By availing himself of Mr. W. Simms' assistance in this very trying adjustment, Mr. Sheepshanks was able to employ all his eyesight on the actual comparisons, which would otherwise have been spent on the adjustment.

bable error, too, is satisfactorily small, scarcely exceeding in any case, or for any observer, one division of the micrometer for a comparison.*

The differences of the results of the several observers are larger than would be expected from the probable error of each; for instance, taking the extreme cases among six observers,

		div.	Probable Error. div.
Mr. Sheepshanks finds bronze 10	= bronze 28	−1·28	0·042
Mr. W. Simms — —	= —	−1·83	0·074
Again,			
Mr. Simms finds bronze 19	= bronze 28	+0·29	0·097
Mr. W. Simms —	= —	−0·41	0·138
And			
Mr. Sheepshanks finds bronze 20	= bronze 28	+0·31	·074
Mr. W. Simms — —	= —	+0·83	·138

Bronze 10 by mistake was measured twice over, and the small probable error of Mr. Sheepshanks is due to the large number of comparisons, at least 400. The number of comparisons by Mr. Sheepshanks is generally three or four times larger than by any other observer. Mr. W. Simms seldom made more than half the number of the other observers. It seems difficult to account for these large differences, except by supposing that each observer sees each division in a manner peculiar to the division, and that we cannot assume the *identity* of the position of a division, as seen by different observers. This indefiniteness is very small, and not likely to affect any useful application, still a limit seems to be shown which, in *optical measures*, no amount of observation in our present state of knowledge can overpass.

The Committee have recommended that of the six bars which are now finally determined, two shall be preserved in public offices, one walled up in some public and fireproof building, one kept at the Royal Observatory, one at the Royal Society, and one, for the present at least, retained in their own hands. This is required for ulterior researches. Twenty-four similar bars have been compared, but their *definitive* lengths are not yet assigned. Several other bars in copper, brass, cast-iron, Low Moor iron, Swedish iron, and cast steel, have been partially compared, and it is intended to make and preserve an accurate type of the yard in each of these metals, thus eluding the practical difficulty of ascertaining the length of bars of different metal sin widely differing temperatures.

* One division of the micrometer = one-millionth of a yard very nearly, or 0·000036 inches. The probable error of 100 observations does not amount to 0·000004 inches in any observer. The Astronomer Royal finds from all the observations, that the probable error of three comparisons is $0^{div.}\cdot55$, or 0·000020 inches for all the observers except the MM. Simms: their probable error is only $\frac{0\cdot55}{\sqrt{2}}$ divisions, or 0·000014 inches. The comparisons are made in sets of three.

A few of these may require further observations, but it is hoped that far the largest portion may be considered as determined, and at the disposal of the Government.

One peculiarity was very clearly marked—the constant tendency of each observer to make the bisection either too short or too full, as compared with another. The bisections of Mr. Sheepshanks are decidedly the shortest, and those of Mr. W. Simms, junr., and Mr. Henderson, the fullest.* The last-mentioned gentleman read each bisection about five divisions higher than Mr. Sheepshanks. The other observers lie between these extremes, Mr. Simms being about half way. A bisection by Mr. Sheepshanks was always declared to be visibly too short by the other observers when they viewed it, and in like manner he always found one of their bisections to be decidedly overdone. No *error* arises from this idiosyncracy, unless the observations of two differing observers should be mixed; but in delicate investigations it seems advisable that the instrumental errors should always be ascertained by the person who makes the observations.

Whether the *touch*-yard, or *end*-measure, which is generally used abroad, will be found less liable to anomaly than the *optical* yard, has not yet been ascertained. It is intended to transfer the optical bronze yard to a touch-yard of cast-steel, and the necessary observations are already commenced. The optical yard has hitherto, in this country, at least, been used in all philosophical inquiries, as in the trigonometrical survey, in measuring the length of the pendulum, &c.; and Mr. Baily's commission, to which Mr. Sheepshanks succeeded, was to verify this form of measure, which is in many respects the most convenient. The commercial measure is a touch-measure, and this form may deserve a preference in some respects: at any rate it will be the duty of the committee to provide a type of each of the greatest possible accuracy, and to give all facilities for comparing and verifying the yard under the two forms.

Some months ago was published the *Second Report of Her Majesty's Commissioners for the Great Exhibition of* 1851, containing proposals for the employment of the surplus funds in their hands. The main proposition, namely, to procure a site at Kensington for a building to be appropriated to many public uses, and, among others, to be partly occupied by the Scientific Societies of the metropolis, was brought by circumstances before the Council. Some remarks made by the Commissioners on the advantages of joining the societies in one locality, if they did not assume the consent of this and other societies, led many to think

* In the microscopes of the standard yard apparatus, parallel wires are used, and the division is placed between them. The observation is pleasant, and not so fatiguing as with cross-wires; but the personal equation is possibly larger. The space between the wires of the right-hand microscope is larger than that between those of the left-hand microscope, and the personal equation is larger in the right-hand microscope, while the probable error of a bisection is not larger.

that such consent could be readily obtained. Perhaps it would be more strictly correct to say, that what was known of the intentions of the Commissioners had created a strong rumour on the subject, which the expressions of the Report were held to confirm. On the simple question of juxtaposition in a locality favourable to the general purposes of our association, the Council, as a body, had never had any discussion, except in one conversation, which ended in an all but unanimous opinion that the Society is now so favourably placed, that no change could be desired. The question of removal to Kensington, for the purpose of obtaining such juxtaposition, had never been brought forward in any shape. When, however, the Report of Her Majesty's Commissioners above mentioned came before the Council, this last discussion was a matter of necessity; and it soon appeared that there was but one opinion, namely, that removal to a distant suburb, or to any suburb, would be in the highest degree detrimental to the Society, if not absolutely ruinous. This opinion the Council expressed in certain resolutions, which were forwarded to Her Majesty's Commissioners, and which have been printed and distributed. As the question will probably receive still more of public attention, the Council invite the attention of the Fellows to the subject. The question of the advantage arising from juxtaposition of the societies in an eligible locality, is one which seems to rest entirely upon the mode which would be adopted of concentrating their libraries and museums. Little business is transacted between the societies as bodies; and that little depends, for facility of transaction, more on the opportunities which the officers have of conferring with each other in the periods intermediate between the meetings, than on the locality in which those meetings take place. All other things being equal, it is clear that the effect of juxtaposition, as such, would be advantageous; it is equally clear to your Council that it would not be so advantageous as to overbalance any moderate disadvantage of local position. Removal to such a distance from the centre of the metropolis as Kensington could not be entertained on any account whatsoever. Of our Fellows who have residences in or near London, two out of them reside east of the meridian of Charing Cross; and this alone would prevent the serious discussion of any proposal to remove as far as the contemplated building at Kensington.

While on this subject, the Council desire to express their sense of the very great benefit conferred on the Society by the Governments which have, one after another, sanctioned this occupation of their present apartments, which they have now held for eighteen years. This feeling is not the less entertained because it is not expressed from time to time: but it would be highly unbecoming to omit some manifestation of it on the present occasion.

The past year has witnessed the discovery of no fewer than *eight* small planets belonging to the group between *Mars* and *Jupiter*, which now includes twenty-three members.

The first was detected on March 17th by Professor Annibal de Gasparis at Naples, and has received the name *Psyche*. The period of revolution is 1848 days, or rather over five years. Professor Gauss has shown that an estimated place of an object (presumed to have been this planet) on the 29th of January previous by Mr. Hind, agrees well with the elements. Its planetary nature, however, was not known to Mr. Hind until the 18th of March, or the night following the actual discovery at Naples.

The second planet was found by Mr. R. Luther, at the observatory of Bilk, near Dusseldorf, on the 17th of April. Upon the proposition of Professor Argelander it has been named *Thetis*. The revolution occupies about 1426 days.

The next planet was discovered by Mr. Hind, at Mr. Bishop's observatory, on the 24th of June, and was named *Melpomene* by the Astronomer Royal. It is one of the nearest of the group, the period being 1269 days, about eight weeks less than that of *Vesta*.

Another planet (*Fortuna*) was found by Mr. Hind on the 22d of August, not far from the ecliptic, in the constellation *Aquarius*. The orbit is very little inclined to that of the earth: the revolution extends to about 1393 days.

The *fifth* planet of 1852 was independently discovered by Professor de Gasparis on September 19th, and by M. Chacornac, assistant to M. Valz at Marseilles, on the 20th of the same month. The inclination of the orbit to the ecliptic is less than that of any other planet, *Uranus* not excepted. M. Valz proposed the name *Massilia* for this object, in which Professor de Gasparis, who has a prior claim to the discovery, appears to have concurred. The period is about 1280 days.

On the 15th of November a *sixth* planet was found at Paris, by M. Hermann Goldschmidt, an historical painter, residing in that city. M. Arago proposed to call it *Lutetia*. It was rather under the ninth magnitude in November, and has now become very much fainter. The revolution occupies 1400 days.

On the night following the last discovery (November 16th) Mr. Hind detected a new planet, with the assistance of one of the ecliptical star-maps at present in course of publication from Mr. Bishop's observatory. It was not much over the 10th magnitude, which is rather beyond the limit of the Berlin Chart. This planet, which performs its journey round the sun in 1816 days, was named by our President, *Calliope*.

The *eighth*, and last planet of 1852, was likewise detected by Mr. Hind on the 15th of December; it was hardly brighter than stars of the 10·11th magnitude, and, being not very far from perihelion, is probably one of the faintest members of the group. On the suggestion of Mr. Bishop it has been named *Thalia*. The period, as nearly as we can ascertain at present, is 1627 days.

Two new comets have made their appearance since our last anniversary, and the remarkable comet of Biela has returned agreeably to prediction.

The first of the new comets was discovered at Marseilles by M.

Chacornac on May 15th, and at Altona by Dr. Petersen on the 17th. There appears to be nothing to distinguish it from a multitude of other telescopic bodies of this nature, whose apparent paths are well represented by the parabola.

The second comet, found by Dr. Westphal at Göttingen, on July 24, and by Dr. Peters at Constantinople about ten days later, possesses a much higher degree of interest, inasmuch as the orbit is undoubtedly elliptical, the period, according to the last calculation, being 61 years. Possibly this object may still be observed with powerful telescopes, owing to its advantageous position in the circumpolar heavens. It would be superfluous to dilate upon the necessity for continuing the observations as long as a glimpse of the comet can be obtained, in order to have the means of predicting the time of its next visit to these parts of space; which will happen, in all probability, before the close of the present century.

The principal component of Biela's double comet would appear to have reached its perihelion three or four days sooner than the date fixed by Professor Santini. The cause of this difference is probably to be sought in the fact of the provisionary elements of M. Plantamour having been adopted as the basis of calculation for 1846. Professor Hubbard of Washington, U. S., thinks there is an error in the computations connected with the perturbations during the comet's last apparition. Both parts of this wonderful body have been re-discovered, though at an increased distance from each other. Signor Secchi, Professor Challis, and the astronomers at Berlin, have observed one or other of these parts, notwithstanding the serious interruptions they experienced from the comet's presence in the morning twilight.

In the last Report of the Council, allusion was made to a new method of computing numerically the special perturbations of planets, for which Physical Astronomy is indebted to Professor Encke. The method, as first explained, applied to the disturbances produced in the rectangular co-ordinates. Dr. Brunnow and Professor Hansen have since introduced modifications of this method, applying to polar co-ordinates. Dr. Brunnow's form of computation gives the numerical values of perturbation in true longitude, radius vector, and latitude; Professor Hansen's, following out the form of theory which he has so successfully employed in other cases, gives the perturbation of mean longitude. It is considered by Professor Encke that, so long as the perturbations are confined to the first order of disturbing forces, these methods are preferable to his original method: they do not, however, apply with the same facility to higher orders of force.

Among the most remarkable applications of astronomical results to determinations of general interest, we may cite the recent calculations of the eclipse of Thales. This, as is well known, formed the subject of a very valuable paper by Mr. Baily (the first of his

astronomical memoirs). Proceeding from the elements of Bürg's tables, Mr. Baily found that no eclipse but that of B.C. 610 could correspond to the historical account: at the same time he pointed out, that the inability of these elements to represent another eclipse of great historical interest (the eclipse of Agathocles) threw doubt on the calculations. Since that time, theory has made important advances, and observations and the reduction of observations have been very greatly improved. In the last year, the demands of chronologers induced two of our members, the Astronomer Royal and Mr. Hind, to recalculate this eclipse. The result has been, that the date of B.C. 610 can no longer be admitted; and the year B.C. 585 appears to be established, beyond the possibility of future doubt, as the veritable date of this remarkable eclipse.

The Astronomer Royal (one of the observers who saw the red projections from, or from behind the edge of the moon, in the total eclipses of 1842 and 1851), with the assistance of Mr. Nasmyth, who contributed the most important part of the apparatus, has endeavoured to ascertain whether red projections from the sun's edge can be seen at times other than those of a total eclipse. For this purpose, a telescope mounted in a large ball-and-socket joint, and with its eye-end projecting into a dark room, has been made to form an image of the sun, the light of which passed through a circular hole (very little exceeding it in diameter) into a blackened box; and it was hoped that any projections from the sun might then be seen depicted on the ring of white paper surrounding the hole. In this manner numerous observations were made during the summer of 1852. The results, however, to this time, are entirely negative; nothing whatever has been seen which could be conceived to arise from any real projection from the sun's limb.

The astonishing discovery (announced in the Council's Report of the last year), that within the inner brilliant ring of *Saturn* there is a faint dusky ring of great breadth, and the still more astonishing discovery by Mr. Lassell, that this dusky ring is partially transparent, have directed the attention of literary astronomers to the history of the former observations of *Saturn's* ring. Much interest has been excited by M. Otto Struve's collection of the observations made at different times from the age of Huyghens, and by his inference that, while the outer edge of the compound bright ring has undergone little alteration, the inner edge has been gradually approaching to the ball of *Saturn*. It appears, however, that, to give these observations their full value, it will be desirable that observations on *Saturn* be repeated with Huyghens's object-glasses, still preserved (it is understood) in the care of the Royal Society.

Very careful drawings of the appearances of *Saturn* have been communicated from time to time to the Society by MM. Lassell, Dawes, and De La Rue, and the last-named gentleman has engraved and distributed copies of the appearances which *Saturn should*

have, according to the data of the *Nautical Almanac*. It is his opinion that in this case the greatest final accuracy is to be arrived at, by directly comparing the graphical results of measurement with the planet itself. Mr. De La Rue has suggested that some additional data in the *Nautical Almanac* would greatly facilitate the accurate representation which is thus required. He has also expressed his intention of embodying in one careful drawing all the results which may be considered as *certainly ascertained* with respect to *Saturn* in the course of the past year by himself and MM. Lassell and Dawes.

Several MS. volumes, containing the observations for the Triangulation of the Cape, have been transmitted to the Royal Observatory, Greenwich. It is greatly to be hoped that active measures may soon be taken for reducing in a systematic way these observations, and for exhibiting to the public their results.

The Society has learnt long since, from the communications of Mr. Maclear, that the bright star *α Centauri* appears to have an annual parallax of about one second of arc. From later communications of the same observer, it appears that the neighbouring star *β Centauri* has an annual parallax of nearly half a second. From the deficiency (at this time) of observations, and from the smallness of the coefficient, we cannot answer with the same confidence for the certainty of this result as for the former; but we have no reason for doubting its accuracy.

It was hoped that, at the last return of Encke's Comet, observations of that body might be obtained at the Cape of Good Hope, which would have been valuable for its theory; and every preparation was made at the Cape Observatory for securing them. The unfavourable position, however, of the comet, and the bad weather which prevailed during the time of its appearance, have greatly diminished the value of the few observations which Mr. Maclear was able to make.

The extended recognition of the value of observations by reflexion from the surface of quicksilver, even for transits and other instruments, for which spirit-levels alone were (till lately) universally employed, has induced several observers to turn their attention to the best methods for quieting the tremors to which the surface of the quicksilver is liable from the most trifling cause of disturbance. In France, MM. Séguin and Mauvais have used, apparently with success, the method of suspending the quicksilver trough by three or four strips of vulcanized caoutchouc. In England, the Rev. C. Pritchard has practically introduced the use of copper or silver troughs, whose surface is amalgamated with quicksilver; the fluid quicksilver in these troughs attaches itself to the amalgamated surface in the same way as water in a wetted dish: it lies in a very thin stratum; and it comes to rest very quickly. The latter method is now in use at the Greenwich Observatory.

Allusion was made in the Council Report of last year to the progress of M. Rümker's labours in the construction of his Catalogue of 12,000 stars. The Council have now the satisfaction to state that this great undertaking, upon which M. Rümker has for many years bestowed so much attention, has finally been completed. It would be difficult to over-estimate the boon which our Associate has conferred upon astronomers by the publication of this catalogue, which is destined to form a durable monument of the perseverance and ability of its author, and of his unwearied devotion to the cause of astronomical science.

During the past year Mr. Grant has completed his *History of Physical Astronomy*, a work the first numbers of which formed his introduction to the Society. It is very seldom that the Council is called upon to notice any work of so peculiar a merit. If Mr. Grant had possessed perpetual access to extensive libraries and peculiar inducements to consult them,—if he had passed many years in historical research, had matured his judgment by long habits of discussion with others of like pursuits, and had learned by small attempts how to venture on larger efforts, the production of a sustained history, of extent and matter such as he has published, would have excited admiration, and would have called forth some remark on the true secret of success in such undertakings. But when it is considered that the author had no more connexion with libraries than any one may command for the asking, and no peculiar calling to tempt him into research,—that he has not reached middle life, and that his opportunities were furnished, as is understood, by the leisure of ill health or convalescence,—that he had, before the publication of his work, little or no sustained opportunity of communication with those who could have helped to direct his reading or form his judgment,—and that he never published anything whatever previously to the appearance of this well-conceived, well-executed, and greatly-wanted history,—the Council, while they state that the work has taken a place among standard books of history from its first appearance, under circumstances which greatly enhance the merit of its author, congratulate the Society upon the prospect of numbering Mr. Grant among its executive members.

With regard to annual parallax, the Council deem it proper to bring prominently before the notice of the Society a remarkable paper, by M. Wichmann on the parallax of the celebrated star *Groombridge* 1830, which has recently been published in the *Astronomische Nachrichten*.* This star, it will be remembered, has the largest proper motion at present known, exceeding 7″ in arc, and, in consequence, various attempts have been made at different places to determine its parallax, the antecedent probability being great that its distance would be easily measurable. The discovery of

* Nos. 841–44.

the proper motion was published by Argelander in the year 1842, and Bessel was induced to give immediate directions to Schlüter, his assistant, to commence a series of measures with the Königsberg heliometer. These measures were begun in October, 1842, and were terminated abruptly near the end of August, 1843, by the illness and subsequent death of the observer. M. Wichmann was then charged with the continuation of the observations and the completion of the investigation, but more pressing duties relating to the observation of planets and comets prevented him from completely fulfilling the wishes of the great astronomer till the year 1851.

The paper under review gives the results of the complete discussion of his own measures, together with those of his predecessors, and occupies more than four complete numbers of the current volume of the *Astronomische Nachrichten*.

In the remarks made upon this paper it is impossible to give more than a sketch of the details of the discussion into which the author is led, neither would the Council be understood to express any decided opinion upon the subjects in dispute between him and other astronomers; the object is only to set clearly, yet briefly, before the Society the chief circumstances as yet elicited by observation respecting this baffling and mysterious star, and to recommend it to the notice of other astronomers who are furnished with instruments enabling them to repeat the measures for the determination of its parallax.

M. Wichmann gives, first, a short historical summary of the attempts of other astronomers. In the year 1846, M. Faye, by transit observations made with the Paris equatoreal, determined the parallax to be 1″·08, and, by this striking result, drew the attention of others to the star. Peters, by discussion of his observations made at Pulkowa with Ertel's circle, in 1843, subsequently determined its parallax to be 0″·226; whilst the author's discussion of Schlüter's observations gave 0″·18. Finally, Otto Struve, by micrometer-measures of differences of north polar distance, made with the great refractor in 1848 and 1849, found a questionable parallax less than 0″·1; a result at variance not only with Faye's value, but with that deduced from Schlüter's observations.

Omitting the author's remarks on the previous discussion between MM. Struve, Faye, and himself, we proceed to explain the method insisted upon by him as proper to be used in his own investigation.

M. Struve had insisted upon the necessity of basing the determination of the parallax upon the *difference* of the observed distances of the star in question from two comparison stars, rather than upon separate discussions of the *absolute* distance of the star from each of the others, on the principle that "the difference between two nearly equal magnitudes can be much more accurately measured than the magnitudes themselves." The author observes that, with regard to the Königsberg heliometer, the greater accuracy

of the differential measures depends less on the truth of this principle than in the observed fact, that in the measure of large distances unavoidable errors occur, which for smaller distances either do not exist at all, or are got rid of as purely casual errors of observation. (In the sequel the author enters minutely into the discussion of the law of increase of errors as the measured distance increases, but we have not space for these details.) To provide, then, for the greatest possible accuracy, the Königsberg observations have been so arranged that the *two* distances (of the star from the two comparison stars) are not observed absolutely, but only *one* of the distances, and the *difference* between it and the other. "The measure of the difference of the distances is thus," he says, "quite independent of the measure of the latter, and the deduction of the result of the difference measures does not at all depend upon our knowledge of the distances of the comparison stars. But," he continues, "from the difference of the distances nothing else is ultimately derived than the excess of the parallax of Argelander's stars above the arithmetical mean of the parallaxes of both the comparison stars. The question, then, whether the latter, in virtue of the observations, are to be regarded as equal to each other, and, therefore, as probably indefinitely small, can only be resolved if, besides the difference of the distances, their sum be also brought into the investigation, or both stars (of comparison) be separately treated; and a reduction of the observations which, as Struve indicates, regards only the difference, could not be regarded as exhausting or setting at rest the subject."

According to this principle, then, the author arranges his equations of condition in two sets, one of which involves the differences, and the other the sums of the measured distances, in the following way. The distances from each comparison star actually measured on each day are reduced to the mean distance for an assumed epoch, and are then strictly compared with the absolute mean distances of the stars for the same epoch, which had been previously deduced, with great care and labour, by means of all the heliometer measures of distance, in connexion with the best meridian observations of the star in question, *Groombridge* 1830. The differences between the separate mean distances for each day, and the absolute mean distances thus computed, are supposed to be mainly due to parallax; but, to avoid the error which might be induced by an error in Bessel's temperature correction for the screw of the heliometer, this latter effect is also introduced as an unknown quantity into the equations of conditions. Taking, then, the two sets of measures, two sets of equations are produced, involving the differences above-mentioned, the relative parallax coefficient for each comparison-star, the temperature coefficient, and a constant.

By subtracting and adding the equations thence arising, two other sets are derived; the first of which involve the *differences* of the measured distances, and the sum and difference of the relative parallaxes, the other set involving the sum of the measured distances and the difference and sum of the relative parallaxes, the

coefficients of the difference of parallaxes in the first set being the same as those of the sum of parallaxes in the second.

In Schlüter's measures, the stars of comparison (a) and (a') were at nearly equal distances on opposite sides of A, the principal star, (a) preceding by 34′ in right ascension, and being about 3′ south of it, while (a') followed by 34′½ in right ascension, and was nearly 5′ north of it. Wichmann added another comparison star (a''), near (a'), in the same parallel with A, and following it by 30′ in right ascension. It thus happened that the first set of equations involved essentially only the sum of the relative parallaxes, and the other only the difference of the same; the coefficient of the difference of parallaxes in the first set, and of the sum in the second, being either nothing or a very small quantity. The author treats with the utmost rigour Schlüter's observations, and deduces the following result from the difference equations; viz. that the half sum of the relative parallaxes for both stars, or the parallax of Argelander's star diminished by the mean of the parallaxes of the comparison-stars, is + 0″·180, with the probable error 0″ 017.

Discussing, in a similar way, his own observations made in 1851, but taking separately the measures of distance, A a' — A a, and A a — A a'', he deduces for this half sum of relative parallaxes, + 0″·0855 in the first case, and + 0″·0892 in the second, the probable errors being 0″·018 and 0″·023 respectively. It may be mentioned here, that the above results are deduced on the supposition (ultimately to be proved) that the difference of the relative parallaxes of a and a', or of a and a'', is equal to 1″·17; but this is of no importance with regard to the substantial accuracy of the above results, since these differences of parallaxes are multiplied in the final values of the relative parallax of Argelander's star by the factors — 0·005 and + 0·0115.

It is clear, then, that the parallaxes of (a') and (a'') are sensibly equal, and of very small amount; and this the author conceives may be received as an incontrovertible fact.

He next proceeds to discuss his equations arising from the sums of the observed distances, and he first observes, that in the discussion of Schlüter's observations, the observations of the sum of the distances of a and a' from A gave a difference of about 2″ between the summer and winter observations, the summer measures being the larger. This result was confirmed by his own observations, both as to the general fact and the amount of the difference, and could only be accounted for by a supposition of parallax in one or more of the stars, or by the effects of temperature. It somewhat unfortunately happens, that the maximum effects of relative parallax and of temperature pretty accurately coincided for the stars in question. Hence it became a very difficult matter to disentangle the separate effects, and it was evident that both effects could not be determined simultaneously from the equations.

The author is on this account obliged to make two separate hypotheses, the first being that the difference of the parallaxes of a and $a' = 0$, and that the whole observed difference between the

summer and winter measures is the effect of temperature; the second being, that the effect of temperature can be correctly computed by Bessel's estimation of it, and that the whole difference is the effect of parallax.

By a rigorous discussion of Schlüter's observations it is found that, on the first hypothesis, the effect of temperature results five times as large as that deduced by Bessel; and on the second hypothesis, that the excess of the parallax of a above that of $a' = + 1''{\cdot}148$.

Since, however, Wichmann is far from thinking that the question is decided by this result, he submits the hypothesis to the far more searching test of his own observations of 1851, and finds that the first hypothesis will not in any degree satisfy the equations, but that on the second hypothesis the difference of the relative parallaxes of a and a' taken as before is $+ 1''{\cdot}186$, and that the equations are perfectly well satisfied. He is so well satisfied with this conclusion, that he announces confidently that "*the existence of a considerable parallax of the star a may be looked upon as demonstrated.*"

Combining then Schlüter's result with his own, he finds the excess of parallax of a above a' to be $+ 1''{\cdot}17$, with probable error $0''{\cdot}08$. He then sums up the facts, which he considers to be adequately proved by his investigation, in the three following propositions:—

1. In the observed differences of the distances $A\,a' - A\,a$ and $A\,a - A\,a''$ there is shown a periodical change, which is of such a nature as to receive a satisfactory explanation on the supposition that the parallax of A is greater by $0''{\cdot}135$ than the mean of the parallaxes of a and a'.

2. A far more marked periodical change, still more accurately deduced, is shown in the observed sums of the distances, and is of such a nature as to receive a perfectly satisfactory explanation on the supposition that the parallax of a is greater by $1''{\cdot}17$ than those of a' and a'', but it cannot be explained by the influence of temperature on the heliometer.

3. The residual errors resulting from the employment of the above suppositions are sensibly greater for the sum measures than for the difference measures; the proportion of the probable error of a difference measure to that of a sum measure being as 1 : 3.

Finally, by assuming that the parallaxes of a' and a'' are insensible, he deduces the following definitive results:—

Parallax of $a = 1''{\cdot}17$
Parallax of $A = 0''{\cdot}71$

In conclusion, the author enters into a critical comparison of the results of Faye, Peters, and Otto Struve with his own, for which we may refer to his paper, as well as for the details of his investigation, which we have not found space to advert to. Indeed, without an attentive perusal of this remarkable paper, no adequate idea can be gained of the skill, labour, and perseverance with which the whole investigation has been conducted.

In concluding our remarks on this interesting subject we would draw attention to the fact, that the large parallax of the star a relatively to the stars a' and a'', is based almost solely upon the circumstance of the heliometer having given for the measured distances of the principal star from a and a' a result greater in summer by a conspicuous quantity than that given in winter, and upon the impossibility of adequately accounting for this *undoubted* fact, by the only obvious supposition excepting that of parallax, that is by the effect of temperature on the screw of the heliometer. It will immediately be suggested to the minds of all astronomers conversant with such measures, that the best *experimentum crucis* that can be used to set this point at rest will be a set of measures of the distance of two small stars similarly situated, where there is no suspicion of a sensible parallax, made as nearly as possible under the same circumstances. It would, indeed, be better to choose stars whose line of direction is nearly at right angles to that of a and a', since then the possible parallactic effect and the temperature effect sought for would have their maxima at different times of the year, and could be brought out separately by means of the same equations of condition.

For the present, though we forbear expressing any opinion upon the degree of certainty of M. Wichmann's definitive results, we cannot avoid expressing our admiration of the extraordinary degree of care, zeal, and ability displayed in his very valuable contribution to parallactic researches.

Catalogue of the Minor Planets at present known, arranged in the order of discovery.

Reference No.	Planet.	Date of Discovery.	Name of Discoverer.
1	Ceres	1801, Jan. 1	Piazzi
2	Pallas	1802, March 28	Olbers
3	Juno	1804, Sept. 1	Harding
4	Vesta	1807, March 29	Olbers
5	Astræa	1845, Dec. 8	Hencke
6	Hebe	1847, July 1	Hencke
7	Iris	1847, Aug. 13	Hind
8	Flora	1847, Oct. 18	Hind
9	Metis	1848, April 26	Graham
10	Hygeia	1849, April 12	De Gasparis
11	Parthenope	1850, May 11	De Gasparis
12	Victoria	1850, Sept. 13	Hind
13	Egeria	1850, Nov. 2	De Gasparis
14	Irene	1851, May 19	Hind
15	Eunomia	1851, July 29	De Gasparis
16	Psyche	1852, March 17	De Gasparis
17	Thetis	1852, April 17	Luther
18	Melpomene	1852, June 24	Hind
19	Fortuna	1852, Aug. 22	Hind
20	Massilia	1852, Sept. 19	De Gasparis
21	Lutetia	1852, Nov. 15	Goldschmidt
22	Calliope	1852, Nov. 16	Hind
23	Thalia	1852, Dec. 15	Hind

Papers read before the Society from February 1852 *to February* 1853.

1852.

[ar. 12. On Baily's Beads, as seen in Eclipse of July 28, 1851. Prof. Powell.
On the Parallax of β *Centauri*. Mr. Maclear.
On a Contrivance for connecting a Clock Motion to an Uniform Rotation. R. L. Jones, Esq.
On Latitude from Extra-meridian Observations. Mr. Diggins.
Account of Solar Eclipse, July 28, 1851. Mr. H. F. Talbot.
Observations of Encke's Comet. Prof. Bond.
Solar Eclipse. J. Hartnup, Esq.
Various Observations. M. Rümker.
Observations of Encke's Comet. Mr. Boreham.
On Solar Spots. Capt. Shea.
On the supposed Period of Revolution of Great Comet of 1680. Mr. Hind.
On the Satellites of *Uranus*. Mr. Lassell.
Ditto ditto Mr. Lassell.
Remarks respecting *Uranus*. Mr. Lassell.
On *Neptune* and his Satellites. Mr. Lassell.
pril 7. Observations of small Planets. Mr. Carrington.
Ditto ditto Mr. Carrington.
On Circle Dividing. Rev. R. L. Jones.
Micrometrical Measurement of ξ *Ursa Majoris*, *&c.* Mr. Miller.
Coefficients of Planetary Perturbations. Mr. Wackerbarth.
Observations of small Planets and Encke's Comet. Mr. Hartnup.
Observations of small Planets. Mr. Carrington.
On the Southern Stars. Lieut. Gilliss.
On a Solar Eye-piece. Rev. W. R. Dawes.
Note on De Gasparis' new Planet. Mr. Hind.
Mars in Opposition, 1852. Mr. De La Rue.
ay 14. Opposition of *Mars*. Mr. Maclear.
De Gasparis' Planet. Dr. Petersen.
Observations of De Gasparis' Planet. Sig. Secchi.
Observations. M. Rümker.
Rectangular Co-ordinates of Luther's Planet. Prof. Chevallier.
On luminous Bodies telescopically visible in Sunshine. Rev. W. R. Dawes.
On the remarkable Phenomenon seen April 8, 1852. Mr. Wilcox.
Ditto ditto Mr. Hall.
Ditto ditto Mr. Ayrton.

Correction of an Error, &c. Lord Wrottesley.
On the Longitude by an Altitude of the Moon. Mr. Aske.
Tables of the Moon's Parallax. Mr. Adams.
June 11. Discovery of *Psyche*. Sig. De Gasparis.
Proposal to note Dimensions of Telescopes. Mr. Nasmyth.
Note on Galle's Discovery of *Saturn's* Dark Ring. Rev. W. R. Dawes.
Letter from the Madras Observatory. Capt. Jacob.
Elements and Ephemeris of *Psyche* and *Thetis*. Mr. Vogel.
Letter to Mr. Grant on estimating Solar Heat. Signor Secchi.
On Vision. Lieut. Hardy.
On a remarkable Appearance at Sunset in April last. Mr. Gray.
On the same Appearance. Captain Shea.
On the Rose-coloured Prominences seen during Total Solar Eclipses. Mr. Babbage.
Nov. 12. Micrometrical Measurements of γ *Virginis*. Mr. Fletcher.
Observations. Mr. Hartnup.
Ditto Mr. Boreham.
Ditto Messrs. Cooper and Graham.
Occultations. Mr. Snow.
On the Comet of 1689. Mr. Vogel.
Observations of Chacornac's Comet. Mr. Bond.
Discovery of *Melpomene*, *Fortuna*, and a Nebula. Mr. Hind.
Discovery of *Massilia*. Dr. Petersen.
Observations, &c., of new Planets and Westphal's Comet. M. Rümker.
Elements and Ephemeris of *Fortuna*. Mr. Vogel.
Observations. Prof. Johnson.
Observations of *Fortuna*. Mr. Boreham.
Planetary Observations. Professor Chevallier.
Observations of *Hygeia*. Mr. Ellis.
Observations of Petersen's Comet. Mr. Maclear.
Ephemeris of *Hygeia*. Professor Chevallier.
On the Appearance of *Saturn*. Rev. W. R. Dawes.
Appearance of *Saturn* at Valetta. Mr. Lassell.
Dec. 10. On a new Portable Transit. Mr. Rothwell.
On an Improvement in Machine for grinding Specula.
Letters to Mr. Sheepshanks and Mr. De La Rue on *Saturn*. Mr. Lassell.
Appearance of *Saturn*. Rev. W. R. Dawes.
On the Brilliancy of the Variable Star α *Orionis*. Mr. Fletcher.
Observations. M. Rümker.

Elements and Ephemeris of *Massilia*. M. G. Rümker.
On a Contrivance for observing Faint Objects. Mr. Pogson.
Proportional Trigonometry. Mr. Timms.
On the Fallacies of Vision. Lieut. Hardy.
On a Mode of supporting Specula. Lieut. Hardy.

1853.
Jan. 14. Discovery of *Thalia*. Mr. Hind.
Letter from Valetta. Mr. Lassell.
Proposal for observing Red Prominences on Sun's Edge. Mr. Nasmyth.
Expression for Kepler's Problem. Prof. Hansen.
Astronomical Observations. Dr. Drew.
Appearance of *Saturn*. Mr. Hind.
Probable Temperature of *Jupiter* and *Saturn*. Mr. Nasmyth.
Discovery of *Calliope*. Mr. Hind.
Discovery of *Lutetia*. M. Le Verrier.
Additional Remarks on Cosmical Speculations. Mr. Dempster.
On the Use of Mercury for Adjustments of Astronomical Instruments. Rev. C. Pritchard.
On Improving the Action of Telescopes. Mr. Grove.
On the Cause of large Quantity of Rain in 1852. Captain Shea.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
Royal Society of London.
Royal Society of Edinburgh.
Royal Geographical Society.
Royal Asiatic Society.
Royal Institution.
Geological Society.
Linnean Society.
Zoological Society.
Cambridge Philosophical Society.
Society of Arts.
Manchester Philosophical and Literary Society.
British Association.
Art-Union of London.
Institute of Actuaries.
Institution of Civil Engineers.
Corporation of Glasgow.
Hon. East India Company.
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London Spring-Water Company.
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L'Académie National des Sciences de l'Institut de France.
Dépôt Général de la Marine.
La Société de Physique de Génève.
Royal Academy of Munich.
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Royal Academy at Brussels.
Royal Academy of Sciences at Göttingen.
Imperial Academy of Sciences at Vienna.
Imperial Observatory at Vienna.
The Observatory at Bonn.
The Observatory at Milan.
The American Philosophical Society.
The Smithsonian Institution.
The Franklin Institute.
The Editor of the Athenæum Journal.
The Editor of the Literary Gazette.

G. B. Airy, Esq.
Don José Arguelles.
Josh. Bateman, Esq.
George Bishop, Esq.
C. Babbage, Esq.
Adml. Sir F. Beaufort.
W. R. Birt, Esq.
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Rev. T. Chevallier.
Professor A. Colla.
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Warren De La Rue, Esq.
M. Daussy.
M. Dean de Luigné.
Geo. Dollond, Esq.
Dr. Drew.
Professor Encke.
Mons. H. Faye.
M. Fayet.
M. A. Gautier.
Dr. B. A. Gould.
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W. Gray, Esq.
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M. C. Rümker.
Prof. G. Santini.
Sig. Scarpellini.
Dr. G. Schweizer.
Signor A. Secchi.
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W. Simms, Esq.
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R. Taylor, Esq.
M. Villarceau.
Prof. P. Volpicelli.
B. Vulliamy, Esq.
T. S. Wilkkinson, Esq.
W. S. B. Woodhouse, Esq.
M. R. Wolf.

Address delivered by the President, J. C. Adams, Esq. M.A. F.R.S., on presenting the Gold Medal of the Society to Mr. Hind.

Gentlemen,—You have heard from the Report which has just been read how much reason we have to congratulate ourselves on the present state and future prospects of our science. Never was there a time when greater vigour and activity were exhibited in the promotion of it. Nor is this activity confined to one country, or devoted merely to one department of astronomy. Whether we regard the introduction of improved instruments and methods of observation, or the more rigorous discussion to which the observations are submitted, the formation of extensive catalogues of stars, the discovery of new members of our planetary system, or the closer and more systematic scrutiny and examination of those which are already known, in every direction we find the most satisfactory evidences of progress.

One of the most prominent features of astronomical discovery for several years past, has been the continual addition of new members to the remarkable group of small planets between the orbits of *Mars* and *Jupiter*, and the year just ended has been distinguished beyond all precedent in this respect.

Since our last anniversary meeting no fewer than eight of these bodies have been brought to light, and the supply seems to be inexhaustible. New discoverers have made their appearance on the field, while those who have already distinguished themselves seem to have acquired a new aptitude in the search.

It is gratifying to find that one of our own body has been the very foremost in this noble career of discovery; and to him, in testimony of our appreciation of his well-directed and successful labours, the Council has awarded the medal, which it is my pleasing duty this day to present.

Skilfully using the excellent instrumental means placed at his disposal by the enlightened liberality and scientific zeal of Mr. Bishop, and in spite of the interruptions occasioned by a climate, the disadvantages of which are peculiarly felt in researches of this nature, Mr. Hind has added no fewer than eight planets to our system, four of which have been found in the course of the past year. After this, I feel that it is unnecessary to add another word in justification of the award of your medal. Mr. Hind's discoveries are of a nature to be understood and appreciated by all; and I shall, therefore, confine myself to a very brief notice of some circumstances connected with them, and to a few remarks on the conclusions to which they seem to point, respecting the constitution of our planetary system.

The first five of Mr. Hind's planets were found by comparing the heavens with the excellent and well-known star maps of the Berlin Academy. These, however, are limited to 15° on each side of the equator, and, therefore, do not include the whole of the region about the ecliptic, which it is so desirable to examine;

neither do they contain stars smaller than between the ninth and tenth magnitudes.

Mr. Bishop, therefore, very soon determined to intrust to Mr. Hind the formation of a series of ecliptic charts, which should contain all stars down to the eleventh magnitude, which were situated within 3° on each side of the ecliptic. Mr. Hind has already begun to reap the fruits of these labours, the planet *Fortuna* having been detected in the course of preparing one of the charts, while *Calliope* and *Thalia* were found by the comparison of two of the completed charts with the heavens.

Eight of these valuable charts have now been published, and I understand that most of the remaining ones are considerably advanced. Other astronomers, particularly Mr. Cooper of Markree, are engaged in the preparation of charts on a similar plan, and the path of future discoverers cannot fail to be singularly facilitated by their means.

The existence of such a numerous group of small planets in the same part of our system has naturally given rise to much speculation respecting their origin and mutual relations. When, instead of the single planet which was expected to fill up the gap between the orbits of *Mars* and *Jupiter*, *Ceres* and *Pallas* were found at very nearly the same mean distance from the sun, Olbers threw out the conjecture that they were fragments of a larger planet which had been rent asunder by some internal convulsion, and that many more such fragments probably existed. If this were the case, he reasoned, they would all, after longer or shorter periods, again pass through the point where the explosion took place, and though the perturbations which they would suffer, would, in the course of time, prevent them from continuing to pass exactly through the same point, yet it might be expected that they would not stray far from it, and that, therefore, the remaining fragments might be found by carefully watching the parts of the heavens corresponding to the two points in which the orbits of *Ceres* and *Pallas* approached towards intersecting.

Although the finding of *Juno* and *Vesta* appeared to give some countenance to this hypothesis, later discoveries have deprived it of much of its plausibility. Several of the orbits are everywhere far distant from each other, and where the contrary is the case, the points of nearest approach occur in various parts of the heavens. Probably one reason why Olbers did not discover more of these bodies, though he continued his examination for many years after detecting *Vesta*, was, that he was induced by his theory to confine the search within too narrow limits.

Several astronomers have endeavoured to find some general relations between the orbits of this group, similar to that imagined by Olbers; but it appears to me that they have only succeeded in showing a kind of general resemblance, indicating rather that similar causes have operated in determining the orbits of these bodies than that they were originally identical.

If we allow ourselves to speculate on the formation of our

planetary system, and adopt the nebular theory, it seems at least as easy to imagine that the nebulous matter, circulating in any particular region about the sun, would, in cooling, collect into many small masses, as that it would all coalesce into one.

Although, as has been stated, there is no single point through which all the orbits nearly pass, yet many of them, taken two and two, approach very closely to each other. In the case of *Astræa* and *Hygeia*, in particular, the shortest distance between the two orbits is less than $\frac{1}{150}$th part of the earth's mean distance from the sun; so that, as M. D'Arrest remarks, the time of their actual intersection cannot be very distant from the present.

One of the most curious circumstances connected with this group is, that there are several cases in which the mean distances are nearly identical with each other. Thus the mean distances of *Ceres* and *Pallas* are so nearly equal, that their order of magnitude is sometimes changed by perturbation. The same remark applies to *Iris* and *Metis*, and also to the three planets, *Astræa*, *Egeria*, and *Irene*.

It should be noticed that this identity of mean distance would not be at all explained by supposing the planets in which it occurs to have been originally one.

There are also some remarkable cases in which the mean motions are nearly commensurable. Thus the mean motions of *Juno* and *Vesta* are very nearly in the ratio of 5 to 6, while those of *Juno* and *Flora* are as 3 to 4, and consequently those of *Vesta* and *Flora* as 9 to 10.*

The extreme smallness of the apparent diameters of these bodies makes it very difficult to determine their real diameters by direct measurement. According to Sir W. Herschel's observations, the diameters of *Ceres* and *Pallas* would not be far from 140 English miles, while Schröter's observations would make them much larger. Stampfer has attempted to determine their diameter by means of their apparent brightness, supposing the reflective power of their surfaces to be the same as that which obtains in the case of *Jupiter*, *Saturn*, *Uranus*, and *Neptune*. This supposition is obviously rather precarious, especially as the reflective power of *Mars* is found to be much less than that of the other planets; but Stampfer's result agrees very closely with the above-mentioned determination of Sir W. Herschel. Several of the more recently-discovered planets appear to be much smaller than these; and it is not improbable that there are many more which, by their excessive minuteness, elude our telescopes altogether. In this point of view, these asteroids would seem to form a connecting link between the larger planets and the aerolites, the cosmical nature of which appears to be pretty well established.

To the physical astronomer these bodies offer problems of great

* The mean daily sidereal motion of *Juno* is 814″·24; that of *Vesta*, 977″·20; and that of *Flora*, 1086″·08. Also $\frac{6}{5} \times 814{\cdot}24 = 977{\cdot}08$, and $\frac{4}{3} \times 814{\cdot}24 = 1085{\cdot}65$.

interest and difficulty. On account of the large eccentricities and inclinations of some of the orbits, methods of approximation which succeed in determining the perturbations of the older planets, become quite inadequate to deal with these, and, consequently, astronomers have hitherto been compelled to have recourse to the method of mechanical quadratures in order to calculate their motions. But although this method may be employed in all cases, and the use of it becomes much simplified by applying it directly to the differential equations of motion, in the elegant manner which has been recently devised by Mr. Bond and Professor Encke, yet it only enables us to follow the disturbed planet, as it were, step by step, and it is therefore very desirable to have a method by which the course of the planet might be traced through an indefinite number of revolutions, and the results of which might be embodied in tables.

Professor Hansen has attacked this very difficult problem with his characteristic originality and skill, and Sir J. Lubbock has also treated the same subject very ably in his tracts on the perturbations of the planets. Much, however, remains to be done before the application of the method of quadratures to these cases can be superseded. It will be quite indispensable to take into account the square and higher powers of the disturbing force.

It may be remarked, however, that the eccentricities and inclinations of the orbits of several of these new planets are so moderate, that there will be little difficulty in calculating their perturbations by the ordinary methods.

The disturbances which these bodies suffer from the action of *Jupiter* are so large as to afford an excellent means of determining the mass of that planet. It was thus that Nicolai found that the value of this mass which had been employed by Laplace and Bouvard was considerably too small,—a result which Mr. Airy afterwards confirmed by direct measures of the elongations of the satellites. Considering the great degree of proximity to each other, to which these bodies sometimes attain, it does not seem improbable, notwithstanding their minuteness, that they may occasionally produce a sensible effect on each other's motions, in which case the astronomer would be able to weigh these minute atoms in the same balance which he has already applied to the larger bodies of our system.

In examining the heavens in search of small planets, Mr. Hind has naturally been led to pay great attention to the variable stars, and he has consequently detected a considerable number of these objects among the smaller stars. Two of these I will mention, which are at opposite extremities of the scale, and which seem to imply the operation of totally different causes.

The first is that remarkable new star in *Ophiuchus* which Mr. Hind noticed on the 27th of April, 1848, as being of the 6th magnitude, and occurring in a spot where he was certain no star even of the 9.10th magnitude had been visible three weeks before.

After attaining to the 4.5th magnitude, so as to be conspicuous to the naked eye, it gradually faded away, and at present it is only of the 11th magnitude.

The other star to which I will refer appears to vary in a similar way to *Algol*. Its period, according to Argelander, is about $9^d\ 11^h\frac{1}{2}$, but for 9 days of this time it shines as a star of the 8th magnitude, then suddenly descends to the 10.11th, and as quickly returns again to the 8th.

Variations of this latter kind appear to be most naturally accounted for by the periodical interposition of an opaque body in its revolution about the star, but those of the kind first-mentioned seem to mock all our attempts at explanation.

In recording these discoveries, it is doubly gratifying to recollect that they emanate from an observatory founded and maintained by a private individual out of pure love of the science and zeal for its advancement. Of the judgment which Mr. Bishop has shown in the selection of his observers, and the choice of objects of observation, there can be no better proof than is afforded by the admirable double-star observations of Mr. Dawes and the planetary discoveries of which we have just been speaking. Mr. Bishop may well feel proud in the consciousness that his observatory has been the means of contributing so largely to science, and has thus become known wherever astronomy is cultivated.

Another subject of congratulation is the manner in which Mr. Hind's services to science have been recognised by the Government of the country. It is sometimes asked, whether the progress of science is best promoted by private or by public means; but the truth is, that there is no such opposition between these modes of advancing it as is implied in the form of the question. In a country where the dignity of science, and the benefits which it confers, are properly estimated, both Government and people will harmoniously co-operate in its support, and each will easily find its appropriate sphere of action. Surely few objects can be mentioned more truly national in their character than the encouragement and reward of scientific discoveries, which at the same time reflect honour on the country, and give so powerful an impulse to the intellectual advancement of the people.

The President then delivering the Medal to Mr. Hind, addressed him in the following terms:—

Mr. Hind,—It is with peculiar pleasure that I present you with this Medal, in testimony of our appreciation of your eminent services to astronomy. The whole world will acknowledge how nobly it has been earned, and will join with us in the wish that your health may long be spared, and that thus you may be able to make many more additions to our knowledge in that field of science to which you have devoted yourself with so much energy and success.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected :—

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G. B. AIRY, Esq. M.A. F.R.S. Astronomer Royal.

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ERRATUM

In the Notice for January.

Page 68, line 14 from top, *for* $\frac{n''}{n'} \frac{R''.\Delta''.\sin Q''}{R'.\Delta'.\sin Q'}$, *read* $\frac{n''}{n} \frac{R''.\Delta''.\sin Q''}{R'.\Delta'.\sin Q'}$

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIII. *March* 11, 1853. No. 5.

G. B. AIRY, Esq., President, in the Chair.

Follett Osler, Esq., Hazelwood, near Birmingham, and the Rev. Stephen Parkinson, St. John's College, Cambridge,

were balloted for and duly elected Fellows of the Society.

Letter from Mr. Lassell to the Rev. R. Sheepshanks.

"I have now to trouble you with the results of my observations upon *Uranus* and his satellites, which have now got too much into the twilight for further observation. In another mail or two I may probably send you a summary of my observations upon *Saturn*, with some more diagrams, recording in detail some of his delicate phenomena, with the dates in order of time. It will include also a series of observations of *Hyperion* and some measures of the diameters of the planet and major axis of his ring. From its voluminous appearance, however, amounting to three or four sheets, I feel some diffidence about sending it off, though I cannot but persuade myself that it may be an interesting record to refer to, of the appearance of an object which is unique and of such surpassing interest.

"I will mention *en passant* a phenomenon of the obscure ring which I have only lately noticed, and which I fear cannot much longer be observed during this apparition: indeed, the sky during the last month, and thus far in the present one, has yielded me only a small fraction of the fine nights I have been used to enjoy in the same period here. The obscure ring appears to me to vary in colour, one end of it being often bluish-gray, while the other is brown. For instance,

Dec. 28	preceding end		bluish-gray	following	brown
Jan. 6	—	—	gray	—	brown
12	—	—	brown	—	gray
25	south preceding		gray	north following	brown
28	—	—	brown	—	gray
Feb. 3	north preceding		brown	south following	gray

"The sky has not permitted me to pursue this very delicate observation so as to arrive at any conclusion about the rotation of the ring; but there is no question about the fact of a difference of colour being sometimes observable.

"I am now beginning to look towards the time of my departure, which I fancy will be about the middle of next month. I don't think we shall leave the island without some regrets for the loss of its splendid skies and marine views, and also for the loss of the agreeable society we have enjoyed here.

"*Observations of the Satellites of Uranus made with the 20-foot Equatoreal at Valetta.*

	Ariel.			Umbriel.		
	Position.		Distance.	Position.		Distance.
	G.M.T.					
1852.	h m	° ′	h m ″	h m	° ′	h m ″
Oct. 15	...	310 0	10 0 10	...	294 0	... 14
16	...	138 0	... ...	...	175 0	... ...
18	11 30	210 0	11 30 ...	11 30	12 0	... ..
29	9 0	...	... ...	...	...	... ..
30	9 2¼	330 40	... 15	9 45	58 59	... 18
31	...	190 0	... 14	...	350 0	... 18½
Nov. 1	...	330 0	... ...	...	5 0	... ...
3	...	113 0	... 10	...	89 0	... 14
4	9 47	331 34	10 21 12·03	9 39	352 35	10 8 20·70
6	9 26	32 48	... ...	9 36	173 35	... ...
10	9 23	188 40	... 11½	9 23	186 40	... 19¼
11	...	44 0	... ...	...	99 0	... ...
12	...	265 0	... ...	...	20 0	... ...
15	9 0	190 56	... 13	0	...	... 19
16	...	52 0	... ...	...	22 0	... ...
18	...	135 0	... 13¾	...	211 0	... 16½
19	...	344 0	... ...	...	146 0	... ...
20	9 10	193 38	... 13¾	8 59	38 44	... 21
Dec. 6	...	93 0	... ...	...	...	... ...
7	...	300 0	... ...	...	...	... ...
8	...	283 0	... ...	...	173 0	... ...
9	8 9	3 58	8 54 13·64	8 28	186 41	9 12 17·43
13	8 14	164 58	8 27 13·18	8 20	203 24	8 38 17·12
1853.						
Jan. 7	...	...	5 57 13·58	...	...	5 46 15·96
11	6 5	348 22	... ...	6 11	210 0	... ...

	Titania.				Oberon.			
	Position. G.M.T.		Distance.		Position.		Distance.	
1852.	h m	° ′	h m	″	h m	° ′	h m	″
Oct. 15	...	168 46	...	30·38	...	332 15	...	40·86
16	10 18	131 55	10 38	27·91	10 10	305 17	10 30	37·85
18	11 41	34 32	12 1	28·76	11 34	239 0	11 50	34·90
29	9 18	317 35	9 37	28·46	9 10	321 52	9 24	40·48
30	10 7	265 8	10 28	25·17	9 56	289 10	10 21	36·19
31	9 50	218 27	10 14	27·39	9 41	256 26	10 1	33·24
Nov. 1	9 50	182 54	10 9	32·44	9 43	224 39	9 59	37·93
2	12 17	146 25	12 32	29·95	12 12	206 40	12 24	42·36
3	9 12	108 46	9 38	25·48	9 2	178 26	9 26	43·19
4	9 29	57 58	10 35	25·62	9 18	157 19	10 30	42·84
5	8 46	17 5	9 3	30·33	8 38	134 48	8 55	38·29
6	9 14	342 14	10 10	31·56	9 6	104 43	9 47	33·58
10	9 11	172 7	9 47	33·08	9 1	353 50	9 31	43·79
11	9 31	138 39	9 50	28·71	9 25	330 23	9 40	42·21
12	8 51	93 51	9 15	25·13	8 43	306 19	9 3	37·94
15	8 51	332 29	9 29	31·19	8 42	211 50	9 15	38·33
16	9 0	291 47	9 30	25·83	8 46	187 26	9 11	43·57
18	9 2	195 45	9 24	30·02	9 3	145 10	9 13	41·43
19	8 51	163 45	9 20	32·17	8 42	118 59	9 4	36·23
20	8 47	129 19	9 30	27·47	8 36	88 20	9 22	34·05
Dec. 6	8 5	178 17	8 28	32·15	7 54	14 22	8 37	41·58
7	8 22	144 29	8 51	30·37	8 13	350 37	8 36	43·68
8	8 36	102 48	8 53	24·95	8 24	328 54	8 42	41·26
9	8 38	50 2	...	...	8 37	304 2	...	...
13	9 0	245 21	8 50	24·54	8 57	184 14	8 43	42·02
1853. Jan. 7	6 40	306 43	6 28	27·74	6 30	238 8	...	...

"*Remarks.*

"Oct. 15. Being the first occasion on which I measured the satellites here, I was greatly struck with their brightness, even the close ones scarce required looking for, and all were well seen with the planet full in the field. *Oberon* the brightest; *Titania* slightly inferior; *Ariel* much brighter than *Umbriel*, which is rather less favourably situated.

"Oct. 16. The planet appearing as a bright sixth-magnitude star, and easily recognisable, I set the telescope upon him for the first time by the naked eye, without using the circles. The close satellites fainter than yesterday.

"Oct. 18. *Titania* decidedly brighter than *Oberon*. Measures of distance taken with difficulty, the sky being variable and often slightly clouded.

"Oct. 29. Only one of the close satellites seen. The nearly full moon within one hour and a quarter of the planet, and more north in declination, puts out the small satellites and renders it not very easy to observe the bright ones.

"Oct. 30. No measures of distance of the faint satellites could be got, the measures of position being, indeed, difficult in the bright moonlight distance of *Ariel*, estimated at six-tenths of the distance of *Titania*; distance of *Ariel* estimated to that of *Umbriel* as five to six.

"Nov. 2. Troubled by thin clouds, which interrupted the observations and prevented more than three measures of each being taken. Close satellites not seen.

"Nov. 4. Atmosphere unusually fine. All the satellites, even the faint ones, measured in position and distance with remarkable ease. *Titania* rather brighter than *Oberon;* measuring power on the micrometer 760, though the satellites were brightest and, on the whole, best seen, with 1018 negative eye-piece. Under these favourable circumstances, I surveyed the planet round to the distance of $2\frac{1}{2}'$ without finding any points of light which might be satellites. Even the faint satellites with this power are bright objects, and not for a moment to be overlooked. The disk of the planet harder and sharper than I have ever seen it before. With 1917 the faint satellites are, I think, even more obvious.

"Nov. 5. *Titania* brighter than *Oberon*, and, therefore, easier to measure. The close satellites not well seen to-night.

"Nov. 6. Interior satellites too faint for measurement in distance.

"Nov. 10. The close satellites in a line with the centre of the planet at $8^h\ 30^m$ G.M.T. *Ariel* three-tenths and a half of the distance of *Titania; Umbriel* six-tenths of the same distance from the planet. Both too faint for measures of distance. *Oberon* brighter than *Titania*.

"Nov. 12. Air unfavourable. Measures taken with much difficulty.

"Nov. 15. Wind troublesome, setting right in the opening of the roof; and the close satellites, though plainly seen with 1018, could not be satisfactorily measured in distance. *Ariel* very much brighter than *Umbriel*. Distance of *Ariel* estimated one-third that of *Oberon*. *Umbriel* about half the distance of *Oberon*. No other stars visible within $3'\frac{1}{2}$ of the planet, with power 1018 (except some which were *ascertained* to be stars).

"Nov. 16. *Umbriel* appears to-night brighter than *Ariel*, which is near its closest approach to the planet.

"Nov. 20. Satellites singularly placed, describing an almost perfect parallelogram.

"Nov. 22. The sky being unfit for measurements of the satellites, I measured the diameter of the planet. The mean of eight measures with the ordinary micrometer power of 760, gave $4''{\cdot}128$.

"Dec. 6. *Titania* in almost the best possible position for measures of distance; and, accordingly, the extreme difference of six measures amounts only to $0''{\cdot}39$. *Umbriel* not seen.

"Dec. 7. Interrupted by clouds; and though the faint satellites had been so bright as to be easily discovered by a non-astronomical friend, who happened to be present, and who had no knowledge of their places, no measures of them were obtained.

"Dec. 8. The disk of the planet very well defined and sharp with 1018; but no ellipticity nor any markings upon his surface can be observed.

"Dec. 9. Obtained measures in position and distance of the close satellites with power 760. Also measured diameter of planet with the same power; the mean of eight measures gave $4''{\cdot}097$.

"Dec. 13. Measured the four satellites in position and distance with 760. *Oberon* considerably brighter than *Titania*. By hiding the planet behind one of the bars of the micrometer, all four satellites were visible at once with 297, the inner ones with surprising brightness, taking into account their extreme proximity to the planet with that power. They instantly vanished when the planet was exposed. The satellites being favourably situated, this experiment was made to ascertain with how small a power they could be seen; and I think this is about the minimum for the close ones.

"Dec. 20. In a very fine sky I was struck with the beauty of the disk of *Uranus* with 297. In scrutinising his surface I frequently suspected a spot near his centre, but could not in the end be certain whether it was real or an optical illusion. Surveyed the planet with 565, with which the bright satellites were instantly seen, notwithstanding the very close neighbourhood of the moon and a rather hazy sky. The field was, however, so full of light, that it would have been in vain to look for the faint ones. The edge of the disk was as hard and sharp with this power as I have often seen the edge of *Jupiter* with a fine refractor of three or four inches' aperture. It was so fine that I applied a power of 778, which it bore admirably; but I could not verify the suspected spot nor

satisfy myself that any phenomena were visible upon his surface; nor that there was any sensible ellipticity of form. I seized the opportunity of measuring his disk; power 760. The mean of eight measures gave 4″·466.

"Dec. 22. The mean of eight measures of the planet's diameter with power 1027 gave 3″·945. Sky rather less favourable than usual.

"Dec. 27. The mean of nine measures of the disk with 760 gave 4″·032.

"1853, Jan. 7. All the satellites well seen with 565, and still better with 1018, which, under good atmospherical circumstances, I generally find the most efficient power for such scrutiny. During this series of observations, though I have very commonly seen small fixed stars in the field with the satellites, I have never seen any which, by magnitude and position, could ever be mistaken for them. I find this peculiarity attend the close satellites, that they do not bear illumination well, and, therefore, cannot be ordinarily seen when the moon is near enough sensibly to brighten the sky. Though very conspicuous objects in the large dark field of a negative eye-piece, the faintest illumination greatly reduces their brightness; and to measure them micrometrically is almost always a work of labour and difficulty.

"Jan. 11. Surveyed *Uranus* with 1018 under a fine sky and admirable definition, the disk appearing perfectly round, and having a remarkably hard and sharp edge. On this occasion, as well as many others which I have not thought it necessary to particularise, I carefully scrutinised the neighbourhood of the planet to the distance of 5′ from his centre, for the discovery of other satellites. In the course of this scrutiny I made many measurements and diagrams of the positions of small points of light, which all turned out to be stars; and I cannot now resist the conviction, amounting, indeed, in my own mind to certainty, that *Uranus* has no other satellites visible with my eye and optical means. In other words, *I am fully persuaded that either he has no other satellites than these four, or if he has, they remain yet to be discovered.*

"The most probable periods of the newly discovered satellites which I have been able to deduce from this series of observations, combined with that of 1851, and the single observation of Nov. 6th, 1847, are, for *Ariel* 2·520378 days, or $2^d\ 12^h\ 29^m\ 20^s{\cdot}66$, and for *Umbriel* 4·144537 days, or $4^d\ 3^h\ 28^m\ 8^s{\cdot}00$. The period of *Umbriel* is almost identically the same as that given in the *Monthly Notice* for March last, page 154, from the observations of 1851; and the difference in that of *Ariel* admits of easy explanation. Owing to the shortness of the period of this satellite, and my being only able to get *estimations* of the position-angles in 1851, and further, having no certain observation to serve as an epoch later than 1847, I could not, from the observations of 1851 alone, ascertain the period so exactly as to be *sure* of the *number* of revolutions elapsing in those four years. In my determination of the period last year I concluded that, in the interval between 1847, Nov. 6·429, and 1851, Nov. 2·500, 580·114 revolutions had occurred, which gave a period of 2·511700 days. It now appears that only 578·114 revolutions could have taken place in that interval, giving a period of 2·520388 days, extremely near to that which satisfies best all the observations.

"Wm. Lassell.

"*Valetta, 8th Feb.* 1853."

Note on the Form of the Planet Saturn. By the Rev. R. Main.

My object in offering a few remarks on this subject, is to obviate any erroneous impressions which might arise from the publication, through mistake, of a paragraph of a letter from Mr. Dawes to Mr. De La Rue, in which doubts are expressed as to the correctness of the resulting ellipticity of the planet deduced by me in a paper printed in the 18th volume of the *Memoirs* of this Society. I have the authority of Mr. Dawes for stating that this portion of his letter was not intended for publication, and that his remarks on my result would have been very much modified if it had been so.

Since, however, any opinion expressed by so accomplished an observer must necessarily carry great weight with it (and in this instance a weight which he would probably not desire), I have thought it right to bring before the notice of the Society a few circumstances which additional research has brought to light, and which have not only confirmed me in my idea of the general correctness of my result, but which make it evident to *me* that it can scarcely be wrong by any significant quantity.

During the time of the visibility of the ring of *Saturn*, the effect of its vicinity to the equatoreal diameter produces so much confusion of the images in the use of a double-image eye-piece micrometer, or of a heliometer, that I doubt whether any trustworthy results could be at all produced, or at all events any results comparable with those deduced from the measures of a planet, without any such obstruction. I have never used a wire-micrometer for measuring *Saturn*, and I therefore do not profess to judge of the accuracy which might be practicable with it under similar circumstances; but my own impression is that the ring would prevent very great delicacy or accuracy in the measures. I, however, lay it down as a first principle, that *all measures made with a double-image apparatus capable of producing a definitive result for the ellipticity of the planet must be made during the time of the invisibility of the ring.*

When my paper was printed, I was actually ignorant of the existence of any extensive series of measures made for determining the form of *Saturn*, and this will be evident to any one reading the remarks in my introduction; and it was some time afterwards before my attention was directed to an excellent series made by Bessel with the Königsberg heliometer in the years 1830–1833, and printed at pp. 169, 170 of the 12th volume of the *Astronomische Nachrichten*. These measures were made with the same object as my own, viz. for the determination of the general form of the planet, as well as of the correct value of the ellipticity. None of them, however, were made during the time of the *total* disappearance of the ring, which took place for the first time at the end of September 1832. The ring reappeared in December 1832, vanished a second time near the end of April 1833, and finally re-

appeared in June of the same year. The only observations recorded by Bessel during this period are included between 1832, Feb. 9 and March 31, and 1833, Feb. 22 and March 29, and during this interval the ring was so small that Bessel desisted from measures of it "because," to use his own words, "the small breadth of it did not appear advantageous for the purpose;" it was, therefore, a very proper time for determining with the heliometer the ellipticity of the planet. On three nights, viz. on Feb. 20, and March 1 and 31 of 1832, he measured the planet in five directions, and convinced himself of its elliptical shape: but we are not concerned with these measures in our present inquiry.

The final results given by Bessel, as deduced from the whole series of measures, and reduced to the mean distance of *Saturn* from the earth (whose log he takes to be 0·9796480), are as follows:—

Diameter of ring.....	= 39″·311	from 44	observations
Equatoreal Diameter of *Saturn*	= 17″·053	70	—
Polar Diameter	= 15″·381	68	—

Hence the ellipticity is $\frac{1}{10\cdot2}$; and this is given by Mr. Hind and others as his definitive result.

If, however, we take the measures made when the ring had nearly disappeared, we shall find a result agreeing almost exactly with my own, and therefore preventing even that trifling doubt which arises from the difference between the above result and my own, $\frac{1}{9\cdot2}$.

I will for the sake of clearness give these measures, which are, when reduced to the mean distance, as follows:—

		Equatoreal Diam.	Polar Diam.	Difference.	No. of Measures.
		″	″	″	
1832, Feb.	9	16·89	15·03	1·86	2
	11	16·97	14·87	2·10	2
	13	16·91	14·95	1·96	1
	14	16·94	15·20	1·74	2
	15	17·18	15·23	1·95	2
	17	17·25	15·28	1·97	2
	20	17·22	15·24	1·98	1
March	1	17·00	15·35	1·65	3
	11	17·12	15·45	1·67	3
	16	16·93	...	...	
	31	17·25	15·34	1·91	3
1833, Feb.	22	16·97	15·06	1·91	3
	23	17·07	15·23	1·84	3
		16·95	15·18	1·77	3
	27	17·14	15·07	2·07	3
March	29	17·26	15·36	1·90	3

whence $$e^2 = 0{\cdot}211 = 1 - \frac{b^2}{a^2}$$

$$\therefore \frac{b^2}{a^2} = 0{\cdot}789$$

and $$\frac{b}{a} = 0{\cdot}8882$$

Hence $$e = 1 - \frac{b}{a} = 0{\cdot}1118 = \frac{1}{8{\cdot}94}$$

The resulting ellipticity deduced from the transverse diameters, as compared with the equatoreal diameters, is, therefore, larger by a trifling quantity than that deduced from the polar diameters, but the agreement between this result, Bessel's result from measures of polar and equatoreal diameters when the ring had nearly vanished, and my own published result, is surprisingly close,—the published result being the *smallest* of the three.

From all these circumstances I cannot help considering the determination of the ellipticity of *Saturn*, from the mean of the above three results, to be as certain as that of any astronomical element whatever, heretofore given.

I would finally remark, that the figure of *Saturn* drawn with my ellipticity, corresponds well with the image of *Saturn* which I saw so frequently during the disappearance of the ring; but that if I trusted solely to my impressions from memory, I should feel inclined to dissent from the result as being too small rather than too large.

Extracts of a Letter from the Rev. Mr. Stoddart, an American Missionary, to Sir John Herschel, Bart., dated Oroomiah, Persia, N. Lat. 37° 28′ 18″, *Long. E. from Greenwich* 45° 5′, *Oct. 29th*, 1852.

" No one has ever travelled in this country without being surprised at the distinctness with which distant objects are seen. Mountains fifty, sixty, and even a hundred miles off, are projected with great sharpness of outline on the blue sky; and the snowy peak of Ararat, the venerable father of mountains, is just as bright and beautiful when two hundred miles distant, as when we stand near its base. This wonderful transparency of the atmosphere frequently deceives the inexperienced traveller; and the clump of trees, indicating a village, which seems to rise only two or three miles before him, he will be often as many hours in reaching.

" In this connexion you will be interested to know that the apparent convergence of the sun's rays, at a point diametrically opposite its disk, which, if I mistake not, Sir David Brewster speaks of as a very rare phenomenon, is here so common that not a week passes in summer, when the whole sky at sunset is not striped with ribbons, very much like the meridians on an artificial globe.

" But it is after nightfall that our sky appears in its highest brilliancy and beauty. Though accustomed to watch the heavens in different parts of the world, I have never seen anything like the splendour of a Persian summer evening. It is not too much to say that, were it not for the interference of the moon, we should have seventy-five nights in the three summer months, superior for purposes of observation to the very finest nights which favour the astronomer in the New World.* When I first came here I brought with me a six-foot Newtonian telescope of five inches aperture, of my own manufacture, and though the mirrors have since been much tarnished, and the instrument otherwise injured, its performance is incomparably superior to what it was in America. *Venus* sometimes shines with a light so dazzling, that at a distance of thirteen feet from the window I have distinguished the hands of a watch and even the letters of a book.

" Some few months since, having met with the statement that the satellites of *Jupiter* had been seen without a glass on Mount Etna, it occurred to me that I was in the most favourable circumstances possible for testing the power of the unassisted eye, and I determined at once to make some experiments on the subject. My attention was, of course, first turned to *Jupiter*, but for a considerable time with no success. It was always so bright, and shot out so many rays, that it seemed quite impossible to detect any of its moons, even at their greatest elongation from the planet. I varied the experiment in several ways, by looking through the tube of a small telescope, from which the lenses had been taken, and also by placing my eye near the corner of a building, so as to cut off the most brilliant rays of the planet, and yet leave the view unobstructed to the right hand or the left; but in neither case could I find any satellite. Sometime after I was sitting on the terrace, as daylight was fading into darkness, and thought I would watch *Jupiter* from its first distinct appearance till it shone out in its full splendour. This time I was exceedingly gratified just as stars of the first and second magnitude were beginning to appear, to see two, extremely faint points of light near the planet, which I felt sure were satellites. On pointing my telescope towards them, my first impressions were confirmed, and I almost leaped for joy. Since that night I have many times, at the same hour of the evening, had a similar view of these telescopic objects, and think I cannot be

* Some five or six years since, Professor Sestini, now of the Georgetown College, near Washington, U.S., made in Rome a series of observations on the colours of stars, and he has lately been engaged in comparing these colours as seen in Rome and in Georgetown, the same telescope being used in both cases; and to prevent deception, he has always made the American observations before consulting the observations made at Rome. He says,—" The results are remarkably uniform; with a few exceptions much more so than I anticipated. So that, were the state of the air to be ranked among the causes of the different colours of celestial objects, it seems to me that there is but little difference between this atmosphere and that of Italy, even with regard to transparency." From this I think I can infer that this climate must be superior for astronomical purposes to that of Italy.

mistaken as to the fact of their visibility. I must, however, add that none of my associates, who at my request have attended to the subject, are *sure* that they detect them, though the most sharp-sighted individual feels some confidence that he can do so. As these friends, however, are not practical observers, their failure to see the satellites does not shake at all my belief that I have seen them myself.

"The time during which these satellites are visible is hardly more than ten minutes. The planet itself soon becomes so bright that they are lost in its rays. I will not stop to discuss the question, in itself a most interesting one, why they are visible at all, when stars of the third and fourth magnitudes are not distinguishable, but merely give the facts in the case, knowing that you will reason on them far better than I can. Both the fixed stars and the planets shine here with a beautifully steady light, and there is little twinkling when they are forty degrees above the horizon.

"Having come to a satisfactory conclusion about the satellites of *Jupiter*, I turned next to *Saturn*. This planet rose so late in the night, that I had not seen it while watching *Jupiter*, and I was very curious to know whether any traces of a ring could be detected by the naked eye. To my surprise and delight, the moment I fixed my eyes steadily upon it, the elongation was very apparent, not like the satellites of *Jupiter*, at first suspected, *guessed* at, and then pretty clearly discernible, but such a view as was most convincing, and raised my wonder that I had never made the discovery before. I can only account for it from the fact that, though I have looked at the planet here with the telescope many times, I have never scrutinised it carefully with the naked eye. Several of my associates, whose attention I have since called to the planet, at once told me in which direction the longer axis of the ring lay, and that too without any previous knowledge of its position or acquaintance with each other's opinion. This independent collateral testimony is very satisfactory to me.

"I have somewhere seen it stated that in ancient works on astronomy, written long before the discovery of the telescope, *Saturn* is represented as of an *oblong* shape; and that it has puzzled astronomers much to account for it.* Am I not correct in this impression; and if so, is it not possible that here on these elevated and ancient plains, where shepherds thousands of years ago watched their flocks by night and studied the wonders of the glorious canopy over their heads, I have found a solution of the question?

"After examining *Saturn* I turned to *Venus*. The most I could determine with my naked eye was, that it shot out rays unequally, and appeared not to be round; but, on taking a dark

* We suspect that Mr. Stoddart labours under an erroneous impression on this point, the observations which ascribed an oblong shape to the planet having been *subsequent* to the discovery of the telescope. It is well known that when Galileo first surveyed the planet with his newly-invented instrument, he found, *to his great surprise*, that it was shaped like an olive.—R. G.

glass of just the right opacity, I saw the planet as a very minute, but beautifully defined crescent. To guard against deception, I turned the glass different ways and used different glasses, and always with the same pleasing result. It may be that *Venus* can be seen thus in England and elsewhere; but I have never heard of the experiment being tried.

"Let me say here that I find the naked eye superior for these purposes to a telescope formed of spectacle glasses, of six or eight magnifying power. This is not, perhaps, very wonderful, considering that in direct vision both eyes are used, without the straining of any one of the muscles around them, and without spherical or chromatic aberration or the interposition of a dense medium.

"As I am an entire stranger, and at the same time am desirous of having these statements make their full impression on your mind, it is proper for me to say that I was formerly for several years a pupil of Professor Olmsted, in New Haven, and have since been admitted to his special friendship: and that I was associated for some time in observations with young Mason, whose early death you have spoken of as a loss to the astronomical world; and though, no doubt, very many persons have more accurate habits of observation than myself, a practice of fifteen years has done much to train my eye for researches like these. You will also bear in mind the great dryness of our atmosphere, indicated by the register, as well as our great elevation. Captain Jacob (*Proceedings of the Edinburgh Royal Society*, vol. ii. No. 36), in speaking of the extinction of light in the atmosphere, says, 'the loss of light in passing from the zenith through a homogeneous atmosphere of 5·2 miles will be ·303. I was much astonished at first discovering that the air had so great absorbent powers, and many ideas are suggested by the fact.'

"My letter is already becoming tedious; but I will venture to trespass on your patience farther by naming a few test objects, which will enable you the better to compare the advantages of our position with your own.

"1. *δ Cephei*. This I have looked at repeatedly with my naked eye; and though I cannot be *sure* that I have seen it double, I put it down in astronomical language as 'strongly suspected.'

"2. The two small stars in the neighbourhood of the pole-star and in the general direction of *γ Cephei* are seen distinctly, and almost every night, as a single point of light.

"3. 4 and 5 *ε Lyræ* are very beautiful and well defined. When lying on my back, the view of these stars, as they have passed near the zenith, has been very similar to that I have often had of *Castor* in a good telescope. There being *no dew* here, it is almost the universal custom for the people to sleep upon the terraced roofs, which gives them a fine opportunity, if so disposed, to gaze on the blue vault above them.

"4. *α Libræ* is seen as two stars in any ordinary state of the atmosphere as readily as *α Capricorni* would be in America.

"5. *Mizar* and *Alcor* in *Ursæ Major*. On looking at these

any favourable night, two faint stars, which must be telescopic in England, are distinctly seen, one on each side of them."

On the Uranography of Saturn, including the Correction of certain Errors propagated generally in Scientific Works on that subject. By Dr. Lardner.

After noticing the views entertained by other writers upon this subject, the author enters into a mathematical investigation of the true form, magnitude, and position of the projection of the rings, as seen from different latitudes of the planet.

If we suppose the observer stationed at any given latitude and a perpendicular drawn from his station upon the plane of the rings, this perpendicular will be the sine of his latitude, while the distance of this perpendicular from the centre of the rings will be the cosine.

If the observer were stationed anywhere in the axis of the planet above the plane of the rings, a visual ray proceeding from his station to one of the edges, and supposed to curve round that edge, would describe a right cone, the axis of which would coincide with that of the planet. The projection of the edges of the ring upon the firmament would be the intersection of the surface of this right cone with the celestial sphere, and would obviously then be a parallel to the celestial equator, whose declination would be the complement of the semi-angle of the cone.

But if the station of the observer be transferred from the axis of the planet to any point on its surface, then the cone formed by the visual ray will no longer be a right cone; it will, on the contrary, be an oblique cone, the obliquity of whose axis to the plane of the rings will be equal to the latitude of the observer. The projection of the rings upon the celestial sphere will be the intersection of this oblique cone with the surface of the sphere, and will, therefore, no longer be a parallel of declination. Different points of its projection upon the sphere will have different declinations; that point which is upon the meridian of the observer above the horizon having the greatest, and that which is upon the meridian below the horizon having the least declination. All the analytical formulæ necessary for the determination of this projection of each of the edges of the rings upon the firmament are given in the paper, and such of the formulæ as are necessary for the purpose are tabulated for every two degrees of latitude within the limits of latitude at which the rings are visible.

It appears from this, that the rings form upon the firmament of the observer zones, the edges of which are at variable distances from the celestial equator, the point of each edge which crosses the meridian being most remote from the equator, and the distance decreasing from that point to the horizon; the point of the edge which is upon the horizon being, therefore, nearer to the celestial

equator, and having a less declination than any other point of the visible portion of the same edge.

From all this it follows that the several edges of the zones formed by the rings are intersected by parallels of declination at various angles, so that each parallel lying within the range of the rings is divided into two, three, or more segments, according to the number of edges which it intersects.

Objects in these parallels are carried by the diurnal motion of the firmament successively under the rings, are occulted by them, and are visible in the interval between them. Some objects are seen at rising below, and at setting above, both rings being visible between them while crossing the interval. The projection of the rings upon the firmament being symmetrical with relation to the meridian, prime vertical, and horizon, the same phenomena in each case, which are developed between the rising of an object and its culmination, are reproduced in a contrary order between its culmination and its setting.

An elaborate investigation of these phenomena relative to the fixed stars, the sun, the satellites, and the planets, are given in the paper; and the solar eclipses in particular, which take place during the winter half-year, are described with minute detail; and the continuance of each class of eclipses is given in terrestrial and Saturnian days.

Among the most remarkable and unexpected consequences of the principles laid down and developed in this paper, are the following:—

At all latitudes under 25° the rings permanently intercept no part of the firmament.

The effects of the diurnal motion, combined with the peculiar position of the rings, are such, that any object whatever which comes above the horizon passes at one time or another between its rising and setting, clear of the rings.

At latitudes between 25° and 45° a certain zone of the heavens, of small breadth, even when greatest, is permanently covered by the inner ring, so that all objects within this zone are perpetually concealed from the observer. This zone at 25° consists of a single parallel, and increases gradually in breadth.

At the latitude of 45° a similar zone begins to be permanently covered by the outer ring, and the width of this zone gradually increases with the latitude. Thus, between the latitude of 45° and that at which the rings wholly disappear by sinking below the horizon of the observer, the only portion of the heavens continually intercepted are two narrow zones, one covered by each of the rings, but less considerably in breadth than the projections of the rings; the breadth of these zones increasing, one from the latitude of 25° and the other from the latitude of 45° to the total disappearance of the rings.

When the sun, as it sometimes does, comes within the declinations which limit these zones of perpetual occultation, it is eclipsed continually from rising to setting.

This is the only case in which a continual solar eclipse is manifested, and this, as appears by the paper, only takes place for intervals of very limited duration and in particular latitudes. The most ordinary solar eclipses which are manifested are such as are of short duration, the sun being carried by the diurnal motion under one or other of the rings, or successively under both of them, and the duration of the eclipses being determined, not as assumed by Dr. Mädler, by the declination only, but by the diurnal motion and the conditions which determine the obliquity of the parallels of declination to the edges of the rings.

It is also shown that no zone of the heavens is constantly visible in the interval between the rings, except between the latitudes 56° and 58°, and there a zone bounded by two parallels very close together is alone visible. At all other latitudes the parallels of declination, which at one altitude pass between the rings, are at other altitudes covered by them.

None of the satellites, except the eighth, are ever eclipsed by the rings, except at or very near the Saturnian equator, where they are intercepted by the thickness of the rings. The eighth satellite, however, having an orbit inclined to the plane of the rings at about 13°, phenomena are developed in its monthly motion, which are analogous to the solar eclipses described above.

The thickness of the rings being unascertained, the author assumes its probable major and minor limits, and investigates the manner in which it would modify the phenomena.

To form a judgment of the interest and importance of this paper, it will, however, be necessary to refer to the paper itself, since it is rather on the details than any general views that the novelty and interest of the points developed in it depends.

Extract of a Letter from Sig. Secchi to the Astronomer Royal.

"I have the honour to announce to you the discovery of a new comet, found yesterday evening at $6^h\frac{1}{4}$ in the constellation *Lepus*, near the star μ. A first comparison with a small star of the 10th magnitude, which was very near the comet, showed that it was moving with rapidity towards the north. Here are the observations, made immediately afterwards by comparison with a star (a) of the 9th magnitude, with a ring micrometer on the equatoreal of Cauchoix.

1858.	Rome Mean Time. h m s		
March 6	8 55 42	Comet's R.A. = (a) + 7·01	Decl. = (a) − 8′ 14″?
	9 43 51	— — = (a) + 1·07	— = (a) −13 29·7

"The position of the star (a) has been obtained relatively to No. 9420 of the H.C. of Lalande, by the intermediate use of another star (e), and is as follows:—

App. R.A. (e) =	4 h 53 m 46·32 s	Decl. (e) = −16° 19′ 20·14″
— — (a) =	4 52 38·77	— (a) = −15 59 1·38

whence we conclude for the comet,

	Rome M.T. h m s	Comet's R.A. h m s	Comet's Decl. South. ° ′ ″
March 6	8 55 42	4 52 45·78	−15 50 47·2
	9 43 51	4 52 39·84	−15 45 41·7

Approximate Hourly Motion.

In R.A. = 7ˢ·4 In Decl. 6′ 33″

"These observations are not corrected for the effects of refraction. The comet was a tolerably fine one. It had a large coma of about 5′, of which the brightest portion, of about 3′ in diameter, exhibited a small luminous point, which was taken for the point of observation: but it was not *too* distinct; it appeared even to have several of these points.*

"On the same evening there was found also a very feeble nebula in the constellation *Leo*. It follows the star γ by $3^m\ 34^s$, and is north of it by about 3′. It is not in the catalogue of Sir John Herschel. The two centres, or most luminous points, are separated by about 3^s in right ascension and by 3′ in declination, and connected by a feeble nebulosity."

"*Rome, March* 7, 1853."

The comet referred to in the foregoing letter was also discovered independently by Prof. Schweizer, at Moscow, on March 8, and by Dr. Hartwig, at Leipsic, on March 10. It was also discovered in America on March 8, as appears by the following message, which was telegraphed from Boston to New York, and transmitted to the Astronomer Royal by the steamer Arabia. The steamer, it is believed, was just on the point of sailing from the latter port when the message arrived.†

"*Boston, 9th March.*

"A comet was discovered at Harvard Observatory last evening, by C. W. Tattle. It is situated about five degrees south of the bright star *Rigel*."

M. D'Arrest, who has calculated the elements of this comet, remarks that they exhibit a distant resemblance to those of the comet of 1664. This would imply a period of about 188 years. It will readily be seen, by simple inspection of a Comet catalogue,

* This would seem to constitute the incipient symptom of a breaking up of the comet into several distinct parts, as in the case of Biela's comet. Hevelius remarks that a similar phenomenon was observed on the occasion of the apparition of the great comet of 1618. In the month of November, when the comet was first seen, the head presented the appearance of a nebulous mass of light. On the 20th of December it appeared to Cysatus at Rome to resemble a number of small stars (*Cometographia*, p. 417). Hevelius states that he himself observed a similar appearance in the head of the comet of 1661 and several other comets.—*Ib.* p. 341.—R. G.

† The original slip, as printed by the telegraph, was sent to the Royal Observatory.

that the agreement in this respect is *closer* than that which subsists between the elements of the new comet and those of any other comet whose orbit has been hitherto calculated. This is all which we are warranted in affirming in the present state of the subject. The question of identity, however, can only be *established* by a rigorous computation of the disturbing forces acting upon either of the two comets during the period comprised between their respective apparitions. The following comparison of the orbits of the two bodies will serve to exhibit the extent of their resemblance. The elements of the comet of 1664 are those given by M. D'Arrest. Both comets are referred to the mean equinox of 1853·0.

	Comet of 1664.	Comet (1) of 1853.
T	1664, Dec. 4·501 M.T. Par.	1853, Feb. 24·2797 M.T. Par.
ϖ	° ′ 133 18	° ′ ″ 153 21 11·7
Ω	83 51	69 49 47·9
i	21 18	20 19 47·0
Log q	0·01104	0·038920
	Motion retrograde.	Motion retrograde.

The comet has been rapidly receding from the earth since its first apparition in Europe. When it passed its perihelion on the 24th of February, its declination *south* was 56° 32′. On the 30th of March, its declination *north* was 9° 22′. It would, consequently, have been very favourable for observation in the southern hemisphere. In all probability it has been observed at the Cape and other places on the southern side of the equator, several weeks previous to its apparition in Europe.

Reduction of the Observations of Eclipses of Jupiter's Satellites, Occultations of Stars by the Moon, and other Astronomical Phenomena, made by the late Rev. Thomas Catton, B.D., Fellow of St. John's College, Cambridge. By G. B. Airy, Esq., Astronomer Royal.

There is a preface to this paper by Mr. Airy, from which the following particulars are extracted.

The observations embrace a period of years extending from 1791 to 1832. The expense of their reduction, and of the printing of the results, has been defrayed from the fund placed by the Lords Commissioners of her Majesty's Treasury at the disposal of the Royal Society, for the aid of scientific enterprises undertaken by private individuals.

Mr. Catton's observatory was situate on one of the towers of St. John's College, Cambridge. Its exact position with respect to the Cambridge observatory was determined geodetically, previous to the reduction of the observations, by Professor Miller and Mr. Adams.

Mr. Catton's observations in right ascension were made with a

transit instrument of 3 feet ·5 inches focal length, and 1^{in}·4 aperture, executed by Sisson. The instrument was originally intended for a simple lens object-glass, but it is believed to have been fitted achromatically before Mr. Catton commenced his observations with it.

The clock had a gridiron pendulum. Its rate was found by Mr. Airy to be steady enough for the wants of extra-meridional observations.

The telescope used for observations out of the meridian appears to have been a 46-inch achromatic. It may be presumed that it had the usual aperture of $3\frac{3}{4}$ inches. An instrument (apparently a quadrant) was also occasionally used for taking altitudes or equal altitudes. It appears from the records of the observations that a divided object-glass was sometimes used for micrometric purposes.

The copy of observations furnished to the Astronomer Royal by Professor Miller consists of two parts.

The first contains meridional observations, principally with the transit instrument, together with notes on the intervals of the wires, the adjustments in collimation, level, and azimuth, &c., commencing with 1791, June 3, and ending on 1832, March 9. Sometimes (but rarely) there are nearly forty observations in one day; but there are numerous interruptions, extending to one or more months, and in one instance even to $2\frac{1}{2}$ years.

The second part contains observations of extra-meridional phenomena, eclipses of *Jupiter's* satellites, occultations of stars by the moon, solar eclipses, and transits of *Mercury*. The observations are accompanied with copious notes, which give a high opinion of Mr. Catton's care in observations. The first observation was on 1791, June 12, and the last on 1831, October 23.

It was evident that the only part of the observations which could possess any peculiar value are the extra-meridional. In the reduction, therefore, the results of which are exhibited in the above paper, the meridional observations have been considered as useful only for the determination of time.

There was exhibited at the meeting, a model of a mounting for a reflecting telescope of large dimensions, designed by the Astronomer Royal, who, at the close of the usual business of the meeting, gave an oral explanation of its construction.

It has been found to be desirable, in making observations with reflecting telescopes, that the same edge of the mirror should always be lowest. The main object of the contrivance proposed by Mr. Airy is to give an equatoreal motion to the telescope consistently with this condition.

The telescope is supposed to be 30 feet long, and to have a four-feet mirror and five-feet tube. It is intended that the mounting of the instrument shall satisfy the following conditions:—

1. The same edge of the mirror will always be lowest.

2. The motion of the telescope will be truly equatoreal.
3. The speed of the telescope for three hours on each side of the meridian will be truly equatoreal.
4. The micrometer-wire in the field of view will continue during the movement of the telescope to pass in the same direction through the same cluster of objects.

The first of these conditions is satisfied by giving the telescope a motion similar to that of an altitude and azimuth instrument. This object is accomplished by supporting the tube of the telescope by means of pivots or trunnions at its centre of gravity, on forks carried by a turn-table of a construction similar to that employed at railway stations. The turn-table is to turn with its centre bearing by a hollow cone upon a centre-pin, and with its circumference bearing upon six or nine wheels. Of these wheels, three will have their centres on firm bearings, and these will determine the position of the turn-table. All the others, as well as the centre-pin, will be carried on levers pressed upwards by springs or weights. The central pin will prevent horizontal unsteadiness.

The motion of the telescope is rendered equatoreal by bridling the elevated end of the tube to an upright pillar placed to the north of the instrument (supposing the place of observation to be in the northern hemisphere), the connexion of the tube with the pillar being such that an imaginary line joining the centre of motion in altitude and azimuth, with the point at which the bridling rod is attached to the pillar, is parallel to the axis of the earth's diurnal motion. The polar bridle is connected at the other end with an apparatus of small bars fixed to the mouth of the tube, and converging to the centre of the opening. It is adjustable in length, according to the polar distance of the object.

Placed to the east of the telescope, parallel to the imaginary polar axis above mentioned, is a real axis, which carries a quadrantal arc revolving in the plane of the equator; and also on the side of the quadrantal arc next the telescope, a graduated meridional arc, having a radial bar moveable along its face. The length of the radial bar is equal to the distance from the centre of motion of the telescope to the centre of the frame of small bars at the mouth of the tube. The radial bar is to be clamped on the meridional arc at the graduation corresponding to the polar distance of the object. The connexion between the telescope and the equatoreal quadrant revolving about the real axis is effected by means of a rod,—whose length is equal to the distance between the centre of motion of the telescope and the centre of the meridional arc,—uniting the upper end of the radial bar which moves upon the meridional arc, with the apparatus at the mouth of the tube. It is manifest from this arrangement that any increase or diminution of the equatoreal motion of the quadrant will be propagated in an equal degree to the tube of the telescope.

The speed of the telescope will, therefore, be rendered truly equatoreal by causing the quadrantal arc (the edge of which for this purpose is toothed) to be worked by a pinion placed on the

spindle of a clock-movement adjusted to sidereal time. When the telescope is in the meridian, the middle point of the quadrant is at the pinion. Hence it is evident that the range of the telescope will extend to three hours on each side of the meridian.

The fourth condition is satisfied by placing the small inclined mirror and the lateral eye-piece on a spindle passing upwards through the centre of the frame-work of small bars at the mouth of the tube, and connecting itself with the polar bridle. By this arrangement the spindle with the mirror and eye-piece are made to rotate within the tube, so that a line seen in the field of view of the telescope will always necessarily maintain the same position with respect to the meridian under view. The spindle is perforated, and when the small mirror is removed, an eye-piece for front view or a photographic plate may be inserted in the perforation.

With respect to an objection which might be urged against this contrivance, in consequence of the obstruction offered to the passage of the light by the apparatus at the mouth of the tube, Mr. Airy remarked that no serious inconvenience would arise from this cause, since the bars composing the apparatus might be constructed of such small dimensions as to occasion only a very trifling loss of light. Nor could the diffraction of light produce any sensible effect, since it is a well-established fact that the influence of this physical principle diminishes continually as the aperture of the telescope is increased.

An objection, which seems to be better founded than either of the above, was offered to this mode of mounting, on the ground that it would be impracticable with such a contrivance to make observations near the zenith. This is a difficulty which presents itself in every form of mounting combining equatoreal movement with an arrangement that keeps the same edge of the mirror downwards. When the question, however, refers, as it does in the present case, to a large reflecting telescope, which is designed mainly for the purpose of making *physical* observations of the celestial bodies, it does not appear that this circumstance is calculated materially to impair the practical utility of the instrument.

It will readily be seen from the foregoing description, that one of the leading features of the mounting proposed in the present instance, consists in the adaptation of its construction to the principles by which the engineer is guided in giving stability to his mechanical arrangements, and in properly adjusting the pressure upon their various points of support. The recent history of the Greenwich Observatory has amply demonstrated, that when instruments of large size are constructed with a due regard to the strains which their different parts have to sustain, their working becomes as practicable as that of smaller instruments, while the advantages arising from such a combination of magnitude, stability, and equable wearing, vastly outweigh those derivable from mere artistic excellence.

It may be mentioned that the model still remains for inspection at the apartments of the Society.

On Four small Stars which had been observed during the preparation of Mr. Bishop's Ecliptical Charts, and have since disappeared. By Mr. Hind.

"In the course of forming our ecliptical charts I have seen, and subsequently lost, four small planets which remained to be rediscovered. I will briefly give the particulars.

"The first occurred upon our chart for 3^h right ascension, and is actually printed on the map. Its position for 1825 is, R.A. $3^h\ 1^m\ 40^s$, N.P.D. 71° 16′, and it was observed between the 8th and 11th of February, 1851, while I was finishing off the map for the engraver. By some oversight I did not discover its disappearance from the above place until August following, when it was useless to search after it.

"The second planet was missed, 1852, Oct. 11, having been entered in the previous spring in R.A. $7^h\ 46^m\ 55^s$, N.P.D. 68° 14′ 5″, for the epoch of the map (1825). It was of the 11th magnitude, and like the last has appeared by an oversight upon the published chart. Unfortunately, I cannot be quite sure of the date when this object was entered. It was certainly between 1852, February 25, and the middle of April; most probably about the beginning of the latter month. The third missing star, which I presume to have been also of a planetary character, was marked on October 25, 1852, in R.A. $3^h\ 23^m\ 40^s$, N.P.D. 68° 30′, near another of the same (eleventh) magnitude upon the map for that hour of right ascension. I missed it on December 8, and subsequently made several fruitless attempts to rediscover it; the want of consecutive fine nights, however, was against my finding the planet again. I suspect it moved away to the north, and that the inclination must have been rather large.

"The fourth object was missed from the chart, Hour IV., last month. I cannot at present recover the date of entry, though it must have fallen during the first eight months of 1852. It was of the eleventh magnitude, and appears upon the published map in R.A. $4^h\ 5^m\ 25^s$, N.P.D. 66° 40′. Did I not know from experience how easy it is to omit noticing the absence of a small star from its place upon the map, I might be inclined to consider the second and fourth as variable stars; the first and third I am satisfied were planets, and the other two I think were probably planets also."

"*Mr. Bishop's Observatory, Feb.* 11, 1853."

The American Ephemeris and Nautical Almanac for the Year 1855. Published by authority of the Secretary of the Navy. (Pp. 474, including an Appendix, pp. 78, royal 8vo.) Washington, 1852.

A fresh proof of the interest felt by our Transatlantic brethren in the advancement of astronomical science, and of their ability in carrying out views of practical utility, has recently been afforded by the publication of the handsome volume above entitled. The

preparation of this work was begun in the latter part of the year 1849, in pursuance of an act of Congress approved on the 3d of March of that year. Lieut. Davies, U.S. Navy, has been appointed Superintendent of the Ephemeris. The theoretical part has been placed under the direction of Prof. Peirce.

The work is divided into two distinct parts. The first part is appropriated to nautical purposes, and is calculated for the meridian of Greenwich. The second part is designed for the promotion of astronomical science, and is adapted to the meridian of Washington.

The nautical part consists of an ephemeris of the Sun and Moon and of the planets *Venus*, *Mars*, *Jupiter*, and *Saturn*, together with tables of Lunar Distances. There are also appended the rectangular co-ordinates used in computing the ephemeris of the Sun.

The ephemeris adapted to each month of the year is comprised in *sixteen* pages.

Page i. is devoted to the Sun. The computations are for *apparent noon*, and the co-ordinates are exclusively those of *right ascension* and *declination*.

Page ii. contains the tables of the semidiameter, horizontal parallax, and time of meridian passage of the Moon. There are columns exhibiting the variations of the horizontal parallax in one hour, at noon and midnight, and also a similar column for the variation of the time of meridian passage.

Pages iii.–x. contain the apparent right ascension and declination of the Moon for every hour of each day of the month, with columns of differences for one minute.

Pages xi.–xvi. are devoted to the Lunar Distances. The arrangement is similar to that employed in the *Nautical Almanac*.

The planetary ephemeris contains the apparent right ascension and declination at Greenwich noon of each of the four planets above mentioned, with the variations of both co-ordinates for one hour; also the time of meridian passage.

The following remarks have been suggested by an examination of the second part of the work.

The logarithms of the quantities A, B, C, D, used in correcting the places of the stars are computed to *five* places of decimals.

The values of the constants of Aberration, Precession, and Nutation employed in these computations are those assigned by MM. Peters and Struve.

The mean places of 100 principal fixed stars are given. These have been derived either from Airy's twelve-year Catalogue of 2156 stars or from the Catalogue of the British Association.

The tables of the apparent places of the principal stars for the upper transit at Washington are arranged similarly to the corresponding tables of the *Nautical Almanac*.

The solar ephemeris is computed both for *mean* and *apparent* noon.

The ephemeris of the Moon's place contains the apparent right ascension and declination at the upper and lower culminations,

with the requisite columns for reduction. There is also a column with numbers for reference to the various stars which accompany the successive culminations of the Moon. A separate list of all the moon-culminating stars is inserted immediately after this ephemeris.

Tables are given for computing the Libration of the Moon, both in longitude and latitude.

The ephemeris of *Mercury* contains the apparent right ascension and declination at mean noon and transit, with corresponding logarithms for interpolation; also the mean solar time of meridian transit.

The ephemeris of *Venus* is similar to that of *Mercury*, except that it exhibits the logarithms of motion in a sidereal minute both in right ascension and declination, with the logarithms of the factors for second differences.

The ephemeris of each of the remaining planets is similar to that of *Venus*, except that the apparent right ascension and declination are calculated for sidereal noon and for *transit*.

There is no ephemeris given of any of the small planets revolving between *Mars* and *Jupiter*.

The rectangular co-ordinates of the Sun are given for every twelve hours, with the corrections for reducing them to the mean equinox of 1855·0. The true longitude, the latitude, and the logarithm of the radius vector, are also computed for the same intervals.

There are tables of the heliocentric co-ordinates of the planets. The table for each planet contains the rectangular co-ordinates, the logarithm of the radius vector, and the longitude of the planet in its orbit. The tables of *Mercury* and *Venus* are calculated for every five days; those of *Mars* and *Jupiter* for every ten days; those of *Saturn* and *Uranus* for every twenty days; and those of *Neptune* for every forty days.

The ephemeris of the Moon was calculated from Plana's theory, combined with Hansen's inequalities due to the disturbing action of *Venus*, Airy's corrections of the lunar elements, and Hansen's secular inequalities of the mean motion of the lunar perigee. An empirical correction by Mr. Longstreth, of Philadelphia, was also used.

The ephemeris of *Mercury* was derived from the theory of the planet given by Le Verrier in the *Connaissance des Temps* for 1848. The ephemeris of *Venus* was founded on Lindenau's tables, applying Mr. Hugh Breen's corrections of the elements (*Mem. Ast. Soc.* vol. xviii.); Airy's Long Inequality (*Phil. Trans.* 1832); and Le Verrier's secular variations of the elements (*Connaissance des Temps*, 1841). The ephemeris of *Uranus* was calculated from Bouvard's ellipse, combined with Le Verrier's perturbations by *Jupiter* and *Saturn* (*Connaissance des Temps*, 1849); and Peirce's corrections of the elements and perturbations due to *Neptune*. The ephemeris of *Neptune* was founded on Walker's orbit and Peirce's theory of its perturbations. In other respects the data of the planetary ephemeris agree very nearly with those adopted in the *Nautical Almanac*.

There are inserted in this part of the work the elements of the solar and lunar eclipses which will happen in the year 1855. Tables are also given, by means of which the solar eclipses may be accurately computed for any assigned place. These tables are founded on a modification of Bessel's formulæ suggested by Mr. T. Safford, jun.

Tables are given for calculating the occultations of all the stars down to the *sixth magnitude*, contained in the Catalogue of the British Association, which can be occulted during the year 1855. The method of Bessel forms the basis of computation.

In addition to the usual list of eclipses, &c. of *Jupiter's* satellites, a complete set of tables is given, by the aid of which the configuration of the satellites corresponding to *any assignable time* may be readily ascertained.

There is a table by Dr. B. A. Gould, containing the geographical positions of the principal observatories which are at present in astronomical activity, or which have been so within the last quarter of a century. The latest and most trustworthy determinations have been employed in its preparation. The following is the position assigned to the Washington Observatory:—N. Lat. 38° 53′ 39″·25 (*Ast. Journal*, iii. 12); Long. W. from Greenwich, as derived from data of the U.S. Coast Survey up to 1852, $5^h\ 8^m\ 11^s{\cdot}2$.

The Appendix, in addition to other matters of less importance, contains Chauvenet's tables for correcting lunar distances and an ephemeris of *Neptune* for the year 1853.

Various individuals have co-operated in the computation of this Ephemeris. The results have in most instances passed under the final revision of Prof. Peirce.

It may be remarked, in conclusion, that the work is beautifully got up. The order of arrangement is neat and lucid; the printing is in a clear bold type, and the paper is of superior quality. The modern style of numerals is used throughout, apparently without any disadvantage.

Upon the probable Identity of the Solar Faculæ with the rose-coloured Protuberances seen during a Total Eclipse of the Sun. By M. Schweizer, Astronomer of the Institute of Geodesy, Moscow. (*Ast. Nach.* No. 849, p. 133.)

In an account of the total eclipse of the sun of August 8, 1850, observed at Honululu, M. Arago threw out a conjecture relative to the possible identity of the solar faculæ with the red protuberances seen around the margin of the moon during the occurrence of the totality.* M. Schweizer was induced by this circumstance to institute a series of observations of the solar faculæ about the time of the total eclipse of July 28, 1851, and to cause drawings of their appearances to be carefully executed by some of the most

* *Comptes Rendus*, April 21, 1851.

expert pupils of the Institute.* The observations were made with a Fraunhofer instrument of 3·3 inches aperture, and a magnifying power of 55. They were commenced on the 9th of July, and were continued till some time after the occurrence of the eclipse. Of the whole series of drawings, a select set, amounting to twelve in number, and representing the appearances of the faculæ during the few days which elapsed before and after the eclipse, were lithographed for the purpose of wider diffusion. A copy of these drawings accompanies the number of the *Astronomische Nachrichten*, containing M. Schweizer's communication. By comparing these drawings with the accounts of the protuberances seen during the total eclipse of July 28, M. Schweizer shows that a remarkable analogy exists between the latter phenomena and those to which the drawings relate. Thus, with respect to the hook-shaped protuberance, seen by all observers of the eclipse on the western limb of the sun, at a distance of about 279° from the north pole of the solar disk, counting towards the east, there was found upon the drawings a *similarly-formed* facula, having the *same position*, which continued from the morning of the 26th of July, when it was first seen, to approach nearer and nearer the margin of the sun's disk, and *had already quitted it on the 28th*, in virtue of the sun's rotation. An equally satisfactory agreement was found to present itself upon comparing the drawings of the faculæ with the descriptions of the isolated red patch seen in the vicinity of the hooked protuberance, and also in several other similar instances. M. Schweizer sums up the results of his comparison in the following terms:—

1. For every group of faculæ which appeared on the *western* margin of the sun's disk within two days *before* the eclipse, and for every group which appeared on the *eastern* margin within a similarly short time *after* the eclipse, and which were demonstrated by the drawings not to be on the sun's disk on the 28th of July, corresponding protuberances were seen.

2. Notwithstanding the rather sudden changes of form to which faculæ are subject, still there were several of them which presented a striking resemblance in this respect to the corresponding protuberances.

3. On the western border of the sun the *configuration* of a hooked, a round, and an elongated protuberance, was exactly the same as that of the corresponding and similarly-formed faculæ.

M. Schweizer remarks, that it is difficult to suppose so striking a coincidence to be purely accidental, and that the probability rather is, that the protuberances are no other than the faculæ which we so often see upon the sun's disk. This conclusion might be objected to by some persons, on the ground that the light of the faculæ is generally more intense than that of the other parts of the solar surface, while, on the other hand, the protuberances shine only by a moderate red light. M. Schweizer, however, seems to

* M. Schweizer states that the pupils of the Institute are habitually instructed in drawing from a very early age.

think that there are circumstances connected with the appearance of the faculæ which serve to obviate this objection.

First, he asserts it to be a well-known fact, that even bright groups of faculæ, as they advance from the eastern border of the sun towards the middle of the disk, now and then vanish, and even seem to reappear in altered forms after they have passed the middle parts of the disk. Again he states, that both himself and his assistants have remarked that the faculæ, although occasionally quite visible when they are *just upon the edge of the sun's disk*, still are, at other times, much less conspicuous in that position than at *some distance* from the edge. He estimates the maximum brightness of the faculæ to be in those parts of the disk which are situate at a distance of from $\frac{1}{2}'$ to $5'$ or $6'$ from the margin. Hence it would appear, that the brightness of the faculæ depends on the position which they occupy with respect to the imaginary line uniting the centre of the sun and the eye of the observer. "Might it not then be easily possible," he says, "that as, in the middle of the sun's disk, so also upon its margin, where the radius forms a right angle with the line before-mentioned, the light of the faculæ should not surpass the rest of the solar surface? nay, might it not descend in intensity to the light of the protuberances? The light of the latter cannot be so inconsiderable, since they continued to be seen, according to some accounts of the eclipse, for a considerable time after the reappearance of the sun.* If this be really the case, it is probable that the cloud-like faculæ merely represent transparent objects, in a small degree or even not at all self-luminous, which allow the sun's light to pass in certain positions, with respect to the eye of the observer, without being themselves visible; in other positions, to transmit to us a maximum of the solar light, either by refraction or reflexion, with such intensity that they appear brighter than the surrounding parts of the surface; and finally, in other positions, to appear with the mild light and colour of the protuberances."

Without taking into account the impossibility of a mere accidental coincidence of the drawings of the faculæ with the protuberances, M. Schweizer sought to establish a connexion between the latter and the penumbræ of the solar spots; but he was unable to arrive at any consistent results. During the days immediately preceding and following the eclipse the number of such penumbræ visible upon the disk was too small to account for the numerous protuberances, and even of those penumbræ or nuclei which *did* appear, not one, however small, was situate *exactly upon the margin of the disk*, as ought to have resulted if they had been identical with the protuberances, and as really occurred in the case of the faculæ. Moreover, the forms of the protuberances do not in anywise resemble those of the penumbræ seen near the margin of the solar disk; while, on the other hand, any person who has observed the

* In Mr. Dawes' account of the eclipse there occurs the following remarkable statement relative to the hooked protuberance:—"To my great astonishment, this marvellous object continued visible for about 5", as nearly as I could judge, after the sun began to reappear." (*Monthly Notice*, vol. xii.; *Memoirs*, vol. xxi.)—R. G.

remarkable that he finds a difference of 1″·3 in the value of the mean parallax, according as one set of tables or the other is employed in the comparison, and not knowing which value to prefer, he adopts the mean of the two for his final result.

" If we consider, however, that the only part of this process which depends on the tables consists in the reduction of the actual parallaxes at the times of observation to the mean value, it is plain that so large a difference in the mean of thirty-four observations can only arise from intolerable errors in the periodic terms of parallax given by one of the two sets of tables.

" The parallax in Damoiseau's tables is given at once in the form in which it is furnished by theory, but that in Burckhardt's tables is adapted to his peculiar form of the arguments, and requires transformation in order to be compared with the former. When this was done, I found that several of the minor equations of parallax deduced from Burckhardt differed completely from their theoretical values given by Damoiseau.

" On further inquiry, I discovered that the difference between Burckhardt's equations of parallax and those of Bürg and Damoiseau had been long since remarked by Clausen, in a comparative analysis of the three sets of lunar tables given in the seventeenth volume of the *Astronomische Nachrichten*, but no notice appears to have been taken of this remark.

" With regard to the parallax, Burckhardt professes to have followed the theory of Laplace; but this agrees very closely with that of Damoiseau, so that errors have evidently been committed by him in the transformation of Laplace's formula.

" These appear to have originated in the following manner :—

" In the formation of Burckhardt's arguments of evection and variation, the *mean* longitude of the sun is employed. Now four of the errors in the coefficients of the minor equations may be accounted for, by supposing him to have erroneously employed the *true* instead of the *mean* longitude of the sun in forming the above-mentioned arguments. In another of these equations, the coefficient is taken with a wrong sign, and in another a wrong argument is employed.

" A strange fatality seems to have attended all Burckhardt's calculations respecting the moon's parallax. In the *Connaissance des Temps* for the year 15 of the Republic, he gives a comparison between the values furnished by Mayer's and Laplace's theories, and he concludes that the error of the former may sometimes amount to 7″.

" But this difference is caused almost wholly by an error in his own transformation of Laplace's expression. In the formation of Mayer's arguments of evection and variation, the *true* longitude of the sun is employed, but Burckhardt appears to have inadvertently used the *mean* longitude instead of it; an error which is the exact converse of the one above noticed with respect to his own tables.

" After examining Burckhardt's tables of parallax, I was naturally led to scrutinise more closely the results of the theories of

Damoiseau, Plana, and Pontécoulant, with respect to the same subject. Although the differences between these were very trifling when compared with the errors of Burckhardt, still they were greater than we had a right to expect, considering the close agreement which existed with respect to the equations of longitude. In the theories of Damoiseau and Plana, the expression for the projection of the moon's radius vector on the ecliptic in terms of her true longitude is required in order to find the relation between that longitude and *the time*, and therefore no pains have been spared to obtain it with accuracy; but in the subsequent operations and transformations necessary in order to deduce the expression for the parallax in terms of the time, the same care has not been employed. In Pontécoulant's theory the time is taken as the independent variable, and, consequently, the analytical expression for the parallax in the form required is obtained immediately, and is developed to as great an extent as the corresponding expression for the longitude; yet in the conversion of his formula into numbers he neglects all the terms beyond the fifth order, so that several of the resulting coefficients are sensibly in error.

"I have endeavoured to supply these defects and omissions.

"In the seventeenth volume of the *Astronomische Nachrichten*, M. Hansen gives the expression which he has obtained for the logarithm of the sine of the horizontal parallax, by means of his new method of treating the lunar theory. I have transformed this expression with the care which its great value deserves, so as to compare it with the results of the former theories.

"The agreement thus found between the several theories is most satisfactory, the difference of the separate values of each coefficient and the general mean rarely amounting to a hundredth of a second. There are only two instances in which this amount is much exceeded. One of these relates to the constant of parallax, the value of which, given by M. Hansen's method, is 0″·06 less than the corresponding value found from the same fundamental data by the other methods, and the second relates to the term whose argument in Damoiseau's notation is $t + z$, the coefficient being 0″·146 according to Damoiseau and Plana, 0″·140 according to Pontécoulant, and 0″·181 according to Hansen.

"The values of the constant of parallax which I have deduced from the theories of Damoiseau, Plana, and Pontécoulant, agree perfectly with one another, and from the particular examination which I have given to this subject, I am induced to place considerable reliance on the result. It is possible that M. Hansen's definitive value of the constant may differ slightly from that which he has given in the paper above referred to.

"From the value of the constant of nutation found by M. Peters, it follows that the ratio of the moon's mass to that of the earth is as 1 to 81·5 nearly. Employing this ratio, together with the dimensions of the earth according to Bessel, and the length of the seconds pendulum in latitude $35^{\circ}\frac{1}{4}$, deduced from Mr. Baily's Report on Foster's pendulum experiments, I find the value of the constant of parallax to be 3422″·325.

"Now Henderson, in the paper cited above, has found the value of the constant, by comparison with Damoiseau's table, to be 3422″·46.

"It should, however, be remarked, that what the tables call the parallax is more strictly the *sine* of the parallax converted into seconds of arc. In Henderson's calculations he has taken the tabular quantity to denote the parallax itself, so that the value found must be diminished by 0″·15, in order to obtain the constant of the *sine* of the parallax. Thus the value deduced in this manner is 3422″·31, a result admirably agreeing with that just derived from theory.

"I have carefully transformed the expression for the parallax given by theory, so as to make it depend on Burckhardt's arguments of longitude, and from the resulting formula Mr. Farley has calculated the tables which are appended to this paper. Constants are added to the several equations so as to render them always positive.

"The minor equations of equatoreal horizontal parallax are comprised in Table I.;

"Table II. contains the equation depending on the argument of evection;

"Table III. that depending on the argument of variation; and

"Table IV. that depending on the argument of anomaly.

"The formulæ employed in their construction are the following, in which

E denotes Burckhardt's argument of evection;
V that of variation; and
A that of anomaly;

and the arguments of the minor equations are denoted by their numbers as in Burckhardt.

0′·34	—	0′·34 cos (Arg. 1)
1′·73	+	1′·73 cos (Arg. 2)
1′·46	+	1′·46 cos (Arg. 4)
0′·87	+	0′·87 cos (Arg. 5)
0′·71	—	0′·71 cos (Arg. 6)
0′·11	—	0′·11 cos (Arg. 7)
0′·62	—	0′·62 cos (Arg. 8)
1′·81	—	0′·05 cos (Arg. 9)
0′·21	—	0′·21 cos (Arg. 12)
0′·16	—	0′·16 cos (Arg. 13)
0′·14	+	0′·14 cos (Arg. 16)
0′·12	+	0′·12 cos (Arg. 23)
0′·10	+	0′·10 cos (Arg. 25)
36′·81	+	37′·22 cos E + 0″·41 cos 2 E
26′·18	—	0′·94 cos V + 26″·34 cos 2 V + 0″·16 cos 4 V
55′ 50′·92		+187′·14 cos A + 10″·27 cos 2 A + 0″·64 cos 3 A + 0″·04 cos 4 A

"In this formula, a few terms have been neglected, the largest of the coefficients of which does not exceed 0″·08.

"The sum of the constants in this formula is 3422″·29, slightly

differing from what is called the constant of parallax, in consequence of the change in the form of developement.

"For the sake of comparison I will here give the formula on which Burckhardt's own tables are constructed, which is as follows:—

0".4	−	0".4 cos (Arg. 1)
0.8	+	0.8 cos (Arg. 2)
0.3	+	0.3 cos (Arg. 4)
0.8	+	0.8 cos (Arg. 5)
1.1	+	0.8 cos (Arg. 6)
0.6	−	0.6 cos (Arg. 8)
1.8	+	1.8 cos 2 (Arg. 9)
0.7	+	0.7 cos (Arg. 12)
1.0	+	1.0 cos (Arg. 13)
43".0	+	37".4 cos E + 0".4 cos 2 E
30.0	−	1".0 cos V + 26".3 cos 2 V + 0".3 cos 3 V
55' 40.0	+	187".0 cos A + 10".2 cos 2 A + 0".5 cos 3 A

"The sum of the constants in this formula is 3420".5.

"The errors of the coefficients of equations 2 and 12 arise from the mistake respecting the formation of the argument of variation before explained, and those of the coefficients of equations 4 and 13 from the similar mistake respecting the argument of evection.

"Equation 6 is taken with a wrong sign, and in the variation equation 3 V appears to be wrongly substituted for 4 V, though I ind that the corresponding term, when reduced to Burckhardt's orm, has a smaller coefficient.

"In consequence of the way in which most of these errors originate, their amount will be generally greatest in March and September, and least about the beginning of January and July, when the sun's mean and true places coincide.

"The total error of Burckhardt's tables may amount to nearly 6", independently of the change in the value of the constant.

"Looking at the accuracy of modern observations, it is easy to imagine to what an extent the value of comparisons between observed and tabular places may be diminished by their being liable to an error of this kind.

"In determining differences of longitude by means of occultations, it is plain that the results may be considerably affected by such an error in the parallax. It has often been remarked, that differences of longitude obtained by means of different occultations are not so consistent with each other as might be expected from the precise character of the observation, and I have no doubt that a great part of the discrepancy is to be attributed to the use of an erroneous parallax.

"Mr. Maclear's observations at the Cape, combined with European observations, would doubtless furnish most valuable materials for a new determination of the constant of parallax, care being of course taken to employ correct tables in the reductions; and such a work would be a useful contribution to astronomy.

"In order to facilitate these and similar objects, Mr. Stratford has calculated the parallaxes from my tables for each Greenwich mean noon in the years 1840–1855, and has thus obtained the corrections to be applied to the corresponding quantities given in the *Nautical Almanac*.

"These corrections are embodied in tables which are appended to the present paper. Subsequently to 1855, the moon's parallax given in the *Nautical Almanac* is calculated from my tables."

Having made these preliminary remarks, Mr. Adams proceeds to give his new tables of the moon's parallax. These are succeeded by the corrections to be applied to the calculated parallaxes in the *Nautical Almanac* for the years 1840–55, as above referred to. The following are the corrections for the year 1853 :—

Day of Mth.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
	″	″	″	″	″	″	″	″	″	″	″	″
1	−0·4	−1·1	+0·5	−0·7	−0·4	−0·8	−0·2	+0·2	−1·0	−2·5	−3·8	−2·8
2	0·3	2·1	−0·6	1·7	1·1	0·6	0·0	0·0	2·0	3·6	4·9	3·2
3	0·4	2·9	1·7	2·5	1·5	0·4	+0·1	−0·4	3·2	5·0	5·5	3·4
4	0·8	3·8	3·0	3·1	1·8	−0·2	0·3	0·9	4·4	5·9	5·6	3·5
5	1·3	4·4	4·0	3·4	1·7	+0·3	0·5	1·5	5·0	6·4	5·5	3·2
6	1·8	4·5	4·7	3·3	1·3	0·5	0·5	2·3	5·4	6·3	4·7	2·8
7	2·3	4·4	4·6	2·9	0·8	0·8	+0·1	2·8	5·1	5·8	4·3	2·3
8	2·8	3·9	4·3	2·4	−0·4	0·9	−0·2	2·9	4·4	5·1	3·5	1·7
9	3·0	3·3	3·7	1·9	0·0	0·7	0·5	2·7	3·3	4·0	2·3	1·0
10	3·1	2·7	3·0	1·2	+0·2	0·6	0·6	2·3	2·5	3·1	1·2	0·3
11	3·0	2·1	2·4	0·9	0·4	+0·4	0·9	1·6	1·6	2·0	−0·5	−0·2
12	2·8	1·8	1·9	0·7	+0·2	−0·2	0·9	1·0	−0·8	−0·7	+0·2	+0·2
13	2·5	1·3	1·4	0·5	−0·1	0·8	1·0	0·8	+0·1	+0·2	0·9	0·5
14	2·2	1·0	1·0	0·7	0·6	1·6	1·0	0·4	0·7	1·3	1·3	0·6
15	1·8	0·9	0·8	1·0	1·5	2·4	1·2	0·3	1·4	2·0	1·3	0·5
16	1·5	1·0	0·7	1·7	2·5	3·0	1·5	−0·2	1·8	2·4	1·1	+0·1
17	1·5	1·0	1·0	2·6	3·4	3·2	1·6	+0·1	1·9	2·3	+0·5	−0·2
18	1·4	1·1	1·4	3·3	3·9	3·2	1·7	+0·1	1·7	1·7	0·0	0·6
19	1·3	0·8	1·6	3·4	4·2	3·0	1·6	0·0	1·2	1·2	−0·3	1·2
20	1·0	−0·4	1·8	3·3	3·7	2·8	1·8	−0·5	0·4	0·6	0·4	1·6
21	−0·6	+0·6	1·7	2·5	2·9	2·7	2·1	0·8	0·1	0·3	0·3	1·9
22	+0·1	1·5	1·3	1·2	2·1	2·7	2·3	1·0	0·0	0·5	0·3	1·9
23	0·8	2·1	−0·1	−0·1	1·4	2·8	2·6	1·2	0·2	0·6	0·2	1·9
24	1·4	2·6	+1·0	+0·7	1·2	2·8	2·4	1·0	0·7	1·0	0·1	1·9
25	1·9	2·7	1·9	1·2	1·4	2·5	2·2	0·7	1·1	1·2	0·3	2·1
26	1·9	2·5	2·5	1·2	1·1	2·0	1·7	−0·1	1·2	1·2	0·4	2·1
27	1·6	2·0	2·7	1·0	0·8	1·6	1·1	+0·4	1·1	0·9	1·0	2·1
28	1·2	+1·5	2·5	0·8	0·4	1·1	0·6	0·6	+0·9	+0·3	1·4	1·9
29	0·6	...	2·1	0·6	0·4	0·8	−0·2	0·7	0·0	−0·8	1·7	1·9
30	+0·1	...	1·3	+0·1	0·5	−0·6	+0·3	+0·5	−1·2	1·7	−2·2	1·8
31	−0·5	...	+0·4	...	−0·7	...	+0·2	−0·2	...	−2·8	...	−1·8

Observations of two of the Satellites of Saturn, and of the Exterior Diameter of the Exterior Ring, made at Valetta. By Mr. Lassell.

Observations of Hyperion.

1852.	G.M.T. h m				″				″
Nov. 1	10 50	precedes	Saturn's centre	—	225·0	South of	Saturn's centre	—	52·9
2	10 54	—	—	—	190·0	—	—	—	67·8
3	11 18	—	—	—	134·8	—	—	—	85·3
4	12 52	—	—	—	81·9	—	—	—	92·0
5	10 31	—	—	—	19·6	—	—	—	97·7
6	11 17	follows	—	—	46·3	—	—	—	92·2
10	11 32	—	—	—	248·9	—	—	—	32·7
11	11 9	—	—	—	258·7	—	—	—	8·5
12	10 54	—	—	—	250·4	North	—	—	18·2
13	10 11	—	—	—	224·4	—	—	—	38·2
15	11 9	—	—	—	101·8	—	—	—	71·6
16	10 11	—	—	—	23·9	—	—	—	75·3
18	10 16	precedes	—	—	132·0	—	—	—	55·4
19	10 17	—	—	—	196·2	—	—	—	37·0
20	10 21	—	—	—	230·7	—	—	—	13·4
27	10 18	follows	—	—	32·6	South	—	—	89·8
29	10 1	—	—	—	154·6	—	—	—	75·3
Dec. 2	8 49	—	—	—	252·6	—	—	—	18·0
3	9 51	—	—	—	256·7	North	—	—	8·0
9	11 15	precedes	—	—	111·6	—	—	—	58·0
13	10 12	—	—	—	225·8	South	—	—	31·8
15	9 29	—	—	—	165·7	—	—	—	67·3
20	8 26	follows	—	—	127·5	—	—	—	74·7
27	8 0	—	—	—	150·5	North	—	—	59·5
1853.									
Jan. 7	8 22	precedes	—	—	75·2	South	—	—	80·5
8	8 18	—	—	—	16·5	—	—	—	87·2
12	7 11	follows	—	—	194·3	—	—	—	53·8
14	6 39	—	—	—	242·7	—	—	—	15·4
15	5 43	—	—	—	242·1	North	—	—	13·3
17	5 57	—	—	—	169·2	—	—	—	50·1

Period deduced from five observations in 1850, compared with the above, 1·297 days.

A rather large ellipticity of orbit is shown by the above observations.

Observations of Japetus.

1852.	G.M.T. h m				″				″
Oct. 30	11 18	precedes	Saturn's centre	—	293	South of	Saturn's centre	—	206
Nov. 1	10 50	—	—	—	195	—	—	—	195
3	10 54	—	—	—	119	—	—	—	195
Nov. 5	10 31	—	—	—	19·6	—	—	—	169·8

1852.	G.M.T. h m									
Nov. 6	11 30	follows	Saturn's	centre	46·3	South of	Saturn's	centre	159·6
10	11 32	—	—	—	217·6	—	—	—	106·0
11	11 9	—	—	—	261·5	—	—	—	89·5
13	10 11	—	—	—	343·9	—	—	—	59·1
15	11 30	—	—	—	409·6	—	—	—	25·4
16	10 31	—	—	—	447·4	—	—	—	7·6
18	10 25	—	—	—	507·6	North	—	—	26·7
19	10 27	—	—	—	517·9	—	—	—	46·4
20	10 36	—	—	—	551·2	—	—	—	68·3
27	10 33	—	—	—	589·3	—	—	—	163·2
29	10 39	—	—	—	568·9	—	—	—	186·5
Dec. 3	10 6	—	—	—	485·8	—	—	—	217·4
13	10 18	—	—	—	113·8	—	—	—	195·4
15	9 29	—	—	—	28·8	—	—	—	175·7
27	8 30	precedes	—	—	431·7	—	—	—	6·5

Measures of the Exterior Diameter of the Exterior Ring of Saturn and of the Polar and Equatoreal Diameters of his Disk.

Diameter of Ring.

1852.		No.	Power.	Reduced to Mean Dist.
Nov. 18	47·4	6	614	40·694
19	47·14	4	...	40·490
29	47·59	7	...	41·152
Dec. 3	46·70	8	...	40·529
20	46·32	5	614	41·046
24	45·66	6	760	40·580
27	45·55	7	614	40·794
		Mean =		40·881

Equatoreal Diameter.

1852.		No.	Power.	Reduced to Mean Dist.
Nov. 19	20·96	3	...	17·990
29	20·94	8	...	18·107
Dec. 3	19·03	6	...	16·515
20	20·01	6	...	17·732
22	19·29	10	760	17·143
24	19·53	7	760	17·409
27	19·29	2	614	17·276
		Mean =		17·453

Polar Diameter.

1852.		No.	Power.
Nov. 29	19·14	10	...
Dec. 3	17·86	5	...
20	17·67	5	...
22	18·45	9	760
24	17·90	6	760

"The measures of the 3d December were taken under the especial determination *not to be in excess*, and probably I should otherwise have taken them somewhat larger. In several instances I have omitted to record the power, but I presume it would be 760, which I most frequently use when the air is in an average state. Comparing the mean of the five measures of polar diameter with the mean of those taken of the equatoreal diameter on the same days, $e = 0{\cdot}078764$, or $\frac{1}{12{\cdot}697}$, *i.e.* the apparent ellipticity, without applying the correction for the present inclination of the axis.

"The measures of the equatoreal diameter and major axis of ring differ considerably from those obtained by Professor Struve at Dorpat. With respect to the ellipticity, I would remark that, without a *perfect* driving motion, it is impossible to measure both diameters of the disk under equal circumstances.

The polar is perfectly easy, while the equatoreal, from being nearly in the direction of motion, is difficult. My driving motion being here somewhat temporarily fixed up is less steady than at home.

"*Valetta, February* 25, 1853."

Extract of a Letter from Mr. Lassell.

"*Valetta, 30th March*, 1853."

"Having concluded my observations here, I beg to hand a series of measurements in position and distance of a number of double stars which I have made, with a view of furnishing to astronomers the means of judging what personal or other equation may be applicable to my micrometrical measurements during my sojourn in this place. All the stars are well known and have been measured before, though of most of them I am ignorant of the results. Some of them are very unequal, and were chosen because they exhibited something like a type of a planet and faint satellite.

"Being all measured by the same observer, with the same telescope and micrometer, in the same place, and nearly at the same epoch, these measures will, I presume, furnish a good standard of comparison of my results with those of other observers.

"They were made, I believe, with neither more nor less care than those of my published observations, except that I paid less attention to accuracy in the *positions* of these stars than in the distances.

"*Measures of Stars made at Valetta for Verification of Measures of Satellites, &c.*

Date.	Star.	Position.	No. of Measures.	Greatest Diff. of Individual Measures.	Magnifying Power.	Distance.	No. of Measures.	Greatest Diff.	Magnifying Power.
1853.									
Dec. 21	γ Arietis	...	...	...	...	8·84	7	0·67	614
	γ Arietis	...	...	...	...	8·67	7	0·63	760
	ζ Orionis	...	...	...	...	56·84	8	0·84	760
28	Rigel	...	...	...	...	9·28	6	0·52	760
Jan.		° ′		° ′					
8	γ Androm. *b, c*	107 51	8	2 28	1027				
	a, b	61 15	3	0 28	1027	10·21	3	0·50	1027
17	γ Androm. *b, c*	111 35	12	3 14	1027				
	a, b	62 59	4	0 51	1027	11·27	5	0·87	760
	Approximate								
	R.A. h m / P.D. ° ′								
Mr. 12	9 29 / 81 7	82 0	3	0 39	614	25·60	8	2·30	614
14	9 20 / 81 7	81 21	9	1 1	614	25·82	9	1·00	614
	*10 24 / 67 7	154 44	4	0 58	760	14·93	9	0·71	760
	†11 27 / 72 19	211 13	4	0 51	760	3·34	8	0·35	760
15	8 31 / 69 46	54 36	6	1 50	760	20·29	8	0·67	760
17	‡8 36 / 71 16	121 7	6	1 11	760	45·82	8	1·25	760
	§9 49 / 69 30	83 36	5	0 58	760	31·83	10	0·73	760

* Σ 1442, vi, 7.8
† Σ 1552, iii, 6.7
‡ δ Cancri
§ Σ 1339, viii, 7.8

Discovery of a New Planet. By M. Chacornac.

(*Extract of a Letter from M. Valz to Mr. Hind, dated April* 9, 1853.)

"I have the honour to announce to you the discovery of a new planet by M. Chacornac, on the 6th of April at 15^h, upon our ecliptic chart for hour xiv. It is of the 9th magnitude, and appears of a bluish tint. I have proposed to call it *Phocea*, which recalls the origin of Marseilles, and to designate it by the symbol 24 enclosed in a circle. At $15^h 40^m$ M.T. it followed 27363 or 27366 of Lalande (which is merely the same star, not double), by $4^m 11^s.9$ of time, and was more to the north by $11' 2''$. In one hour its retrograde motion in right ascension was only 1^s, and it had just passed the ascending node on the preceding day. On the following night, the sky being overcast, it was impossible to obtain a better determination of the direction of motion. On the 8th of April, at $11^h 58^m$, it followed No. 16 of zone 205 of Argelander, by $1^m 57^s$ of time, and was more to the north by $4' 22''$. It thus described about $36'$ in 44 hours, under an inclination of 55° to the ecliptic, indicating thereby a very inclined orbit. I was induced by this circumstance to calculate the inclination upon the hypothesis of the circle, and found it to be 28° 7′; I also found the longitude of the ascending node to be 214° 48′. On the 5th of April, at 11^h, the planet would have passed through the node at a distance of 2·4 from the sun; but this can only be regarded as a rough approximation. On the 8th of April, at 13^h, the planet completely occulted a star of the eleventh magnitude, which may serve to procure a very accurate determination of its position.

"I herewith send you the elements which I have obtained for the last comet, which would appear to be the same as that of 1664, but very much enfeebled, if one may judge from the phenomena accompanying their respective apparitions."

Pass. Per. Feb. 24·067, 1853, M.T.

Dist. Per.	1·0761
Long. Per.	154° 49′
Long. Asc. Node	61 33
Incl.	18 32

Motion retrograde.

Discovery of a New Planet. By Prof. De Gasparis.

(*Extract of a Letter from Professor De Gasparis to Mr. Hind, dated Naples, April* 10, 1853.)

"It is probable that you have already discovered the new planet, very near the ecliptic, which I discovered on the 5th of

April at Naples. I owe this discovery to the disappearance of a small star of the eleventh magnitude, which I had observed on the 5th of April, 1851. Fixing my attention upon the surrounding region, I took note of a small object (under the twelfth magnitude), the proper motion of which I recognised on the following evening.

" Here are the positions of the new planet :—

1853.	Naples M.T. h m s	R.A. h m s	Decl. ° ′ ″
April 6	8 55 34	11 4 17·75	+6 48 24
7	9 16 48	3 50·15	50 42
8	8 55 54	25·48	52 49
9	9 19 3	11 3 2·01	+6 55 0 "

Discovery of a New Comet. By M. Schweizer of Moscow.

A comet was discovered at Moscow by M. Schweizer, on the 4th of April, 1853. It is a small, round, telescopic comet, without any tail. M. Schweizer estimates its diameter to be about 3′; he adds that he perceived from time to time indications of a nucleus. The following are its observed positions, as assigned by him :—

1853.	Moscow M.T. h m	Comet's R.A. h m s	Comet's Decl. ° ′
April 4	15 0	20 3 20	+13 4
5	14 0	20 4 25	+13 1

Note from Mr. De La Rue.

On referring to my notes relating to *Saturn*, I find that my measures of his disk were made on Nov. 19, 1852. On reducing the observations carefully, I have discovered an error which somewhat alters the mean values communicated to Mr. Dawes, and hence the ellipticity deduced therefrom. The following are the corrected numbers :—

Polar Diameter.	Equatoreal Diameter.
″	″
18·68	20·58
18·51	19·89
18·68	20·58
18·85	
18·68	20·35

the ellipticity is, therefore, $\frac{20{\cdot}35 - 18{\cdot}68}{20{\cdot}35} = 0{\cdot}082064 = \frac{1}{12{\cdot}1856}$

April 2, 1853.

Extract of a Letter from the Rev. T. W. Webb to Capt. W. H. Smyth, R.N.

"During the month of April 1852, while I was examining with my $5\frac{1}{2}$-feet achromatic the colour of the star β in this constellation (*Coronæ*), and comparing it with the observation in your most valuable Bedford Catalogue, the remark which you have there made, that this star, though destitute of any Arabic name, is unquestionably the brightest in that asterism, induced me to compare it pretty attentively with its neighbours. I could not, however, at that time, estimate the star in question as the 'lucida'—on the contrary, their order of brightness, to the naked eye, as well as through a common concave eye-glass correcting my near sight, appeared to be evidently $\gamma, \delta, \beta, \alpha$. This was on April 13, 1852.

"A subsequent examination, on the night of April 19, in which, however, the concave eye-glass was not employed, confirmed this estimate. I have not as yet repeated the observation during the present season, but shall be interested in finding whether these stars still preserve the same comparative brightness to my eye, and still more in knowing whether you will agree with me; in which case, perhaps, a decrease of light in the star β, or less probably, an increase in the stars γ and δ, may be supposed to have taken place since the date of your observation in 1831.

"While I am on this subject I may, perhaps, mention, that I have found estimates of relative brightness between stars in the same telescope field, where there is no very obvious disparity, facilitated, or at any rate confirmed, by throwing the instrument a little out of focus; since the comparison between disks of sensible magnitude is rather more easy than between mere points: at any rate, there may be some advantage in its being presented to the eye under a fresh aspect. This method, however, appears still more useful in its application to the determination of the absolute brightness of minute bodies presenting visible, but unequal, disks, such as the satellites of *Jupiter*, where differences in apparent magnitude may confuse the accurate perception of intrinsic splendour. Thus, too, the absolute quantity of light emitted by a small planetary disk, or minute nebula, may be compared with that of the surrounding stars, if any suitable for the purpose are comprised in the same field.

"*April* 6, 1853."

There was exhibited at the Meeting a modification of the model of a mounting for a large reflecting telescope, which had been proposed by the Astronomer Royal at the previous meeting, and of which a brief description was given in the last number of the *Monthly Notices*. In this altered form of the mounting, the polar bridle, instead of extending to the centre of the opening of the telescope, is attached to the back of the tube, by which means the

quantity of framework at the mouth of the tube is considerably diminished. The end contemplated in making this alteration was to obviate any objections which might be urged against the contrivance on the ground of the loss of light, or the injurious effects of diffraction, occasioned by the apparatus of bars at the mouth of the tube: Mr. Airy himself, however, appeared to prefer the original form of mounting to this new modification of it.

On a Mounting for a large Reflecting Telescope.

By Mr. W. Simms, jun.

The Astronomer Royal having at the last meeting called the attention of the Society to a model of a form of mounting for a large reflecting telescope, and at the same time expressed a desire that something better might be devised, I venture to submit to the Society a plan for a similar purpose, which I believe to exhibit some improvements.

In the accompanying model, the turn-table or altitude and azimuth motion is similar to that employed by the Astronomer Royal; the azimuth motion being effected by the table turning upon a strong vertical spindle resting on a conical socket at the lower end; and the altitude motion by a short transverse axis on one side of the telescope. Upon the same side a semicircle is mounted, turning upon two pivots in the direction of its diameter, the centre intersecting the line of the transverse axis; this is the declination arc. A polar axis with the hour-circle at its upper end is carried by a polar support in the usual way, its lower end being the centre of the declination arc which turns there upon an axis: now if the declination arc is clamped to the polar axis, the motion of the instrument will be entirely governed by the clock-work acting upon the hour-circle, precisely as in an ordinary equatoreal instrument, and a considerable range of equatoreal motion obtained, extending to eight or nine hours on each side of the meridian, the motion being as much under control near the zenith as in the lower altitudes, which is one of the improvements intended to be pointed out.

Another arrangement shown in the model is that of the eye-piece, which is fixed to the side of the telescope, so that the observer will not be required to change his position, however great a change may take place in the position of the telescope. A prolongation of the longitudinal axis of the declination arc is connected by means of bevel wheel-work with the eye-tube which is made to rotate, so that the position of a wire in the field would correspond with the equatoreal motion of a celestial object, as in the case of the eye-piece devised by the Astronomer Royal, which is made to rotate through part of the circumference of the mouth of the telescope.

On the Mathematical Theory and Practical Defects of Clock Escapements, with a Description of a new Escapement, &c. By James M'Kenzie Bloxam, Esq.

In this communication the author gives a very complete theory of the dead beat and the remontoire escapements, and shows that in some cases the remontoire is preferable. He also describes a remontoire gravity escapement, which appears to possess some peculiar advantages.

On the Classification of Comets and the Distribution of their Orbits in Space. By Dr. Lardner.

The activity and zeal of observers and computers have of late years supplied vast additions to the knowledge of cometary astronomy. The elements of the orbits through which these bodies have moved through the system have been especially multiplied, and it might be naturally expected that if their orbits like those of the planets be regulated by any particular law in relation to their positions in space, either with reference to each other or with reference to any invariable plane passing through the centre of the sun, such law would be discoverable. It cannot fail, therefore, to be both interesting and useful to review and compare the numerous data thus supplied, with the especial view to the discovery of a law governing the position of the cometary orbits in space, if any such law exist; and if no such law exist, to demonstrate the remarkable fact that comets move in all conceivable directions, and in all conceivable planes, without any greater prevalence in any one direction or in any one plane than in any other.

The catalogues of Pingré and his contemporaries have been of late considerably enlarged by German astronomers, and a very complete one was published within the last few years by Dr. Mädler, in his elementary work on astronomy. This, however, has been lately continued to a much more recent date, and enlarged by the interpolation of several ancient comets, whose elements have been recovered from various traditions and historical records by the zeal and activity of Mr. Hind, to whom science is so largely indebted for his discoveries in this and other branches of practical astronomy.

Taking this enlarged catalogue of Mr. Hind as the most ample repertory extant of cometary data, we find that we are in possession of the elements of the motions of 207 comets. It is true that those which appeared before the middle of the seventeenth century can only be regarded as approximations, some of which may vary from the truth within considerably wide limits, and a few of the comets observed since that date have appeared for such periods, and under such disadvantageous conditions for observation as to supply rather imperfect data; still, on the whole, it may be considered that we have before us the elements more or less exact of the orbits

of 207 comets, whereon to base inductive reasoning directed to the discovery of the laws which govern their position, if any such exist.

Of these 207 comets, it appears that 40 move in ellipses, 7 in hyberbolas, and 160 in parabolas.

The 40 comets to which elliptic orbits have been assigned may be conveniently classified in three groups, as follows:—

1st group—13 comets, whose aphelia are included within *Saturn's* orbit.

2d group—6 comets, whose mean distances are nearly equal to that of *Uranus*.

3d group—21 comets, whose mean distances exceed in a greater or less proportion the extreme limits of the solar system.

First group of elliptic comets.—The 13 comets composing this group are as follows:—Encke, Biela, Faye, De Vico, Brorsen, D'Arrest, Clausen (1743), Burckhardt (1776), Lexell (1770), Blainpain (1819), Pons (1819), Pigott (1783), Peters (1846).

By reference to Mr. Hind's table, it will be perceived that these 13 comets retain in a striking manner many of the characteristics of the planetary motions; they are all, without a single exception, *direct*; their inclinations (with one single exception) are all included within the planetary limits, and even the exceptional comet does not much exceed the inclination of *Pallas*; the excentricities, though of an order of magnitude greatly exceeding even that of *Juno* (the most excentric of the planets), still are incomparably less than those of the other comets. While the aphelia of these orbits generally lie a little within or without the orbit of *Jupiter*, their perihelia are included between the orbits of *Mars* and *Mercury*, the only exception to this being Peters' comet, whose perihelion lies beyond the orbit of *Mars*, while the aphelion extends to the orbit of *Saturn*.

Second group.—Comets of Halley, Pons (1812), Olbers (1846), De Vico (1846), Brorsen (1847), Westphal (1852).

Of the 6 comets which compose this group, 5 have direct motion, that of Halley alone being retrograde. The orbits, however, in all other respects differ in a striking manner from those of the first group, and retain no other trace of resemblance to the planetary orbits. The excentricities are great compared with those of the first group, and of an order to efface all analogy whatever with the planetary ellipses. Their inclinations also range far beyond the planetary limits.

It would, therefore, appear, that this group holds an intermediate place between the first group and the more numerous classes which we are now about to notice.

Third group.—Comets of 1680, 1683, 1763, 1769, 1780, 1793, 1807, 1811 (A), 1811 (B), 1822, 1825, 1827, 1830, 1840 (A), 1840 (B), 1843, 1844, 1845, 1846 (A), 1846 (B), 1849.

Of the 21 comets composing this group, 11 are retrograde and 10 direct.

It appears, therefore, that the tendency to direct motion, which

prevails with a single exception in the first two groups, is entirely effaced in the last; no tendency, on the other hand, however, being discoverable towards the other direction, since the proportion of 11 to 10 in favour of retrograde motion must be regarded as casual.

Fourth group.—Comets of 1729, 1771, 1774, 1818, 1824, 1840, 1843.

Of the 7 hyperbolic comets composing this group, 6 are direct; but among their orbits there appears to be no other prevalent character.

Fifth group.—This group consist of 160 comets, whose orbits are sensibly parabolic.

The distribution of these bodies with relation to the direction of their motion is as follows:—

Direct	70
Retrograde	86
Unascertained	4
	160

This would indicate a slight tendency in favour of retrograde motion, but if we take into account the entire number of comets whose orbits are ascertained this tendency will then disappear, for we shall then have,

Direct	94
Retrograde	99
Unascertained	4
	207

This difference in favour of retrograde motion must be considered as casual, and, so far as the data now before us extend, it must be inferred that the comets taken generally, unlike the planets, show no prevailing tendency either to direct or retrograde motion, but that the elliptic comets of the smaller excentricities show a decided tendency to direct motion.

To determine whether the planes of the cometary orbits are characterised by any prevailing tendency towards a particular inclination, it will only be necessary to ascertain the number of orbits whose inclinations are included between certain definite limits.

It appears from a table which I have formed, exhibiting the proportion of comets in every 100, whose inclinations are included within every successive 10° of the quadrant, that the planes of the orbits of the comets have a decided tendency to crowd around an inclination of about 45°, or to take positions which would be those of tangent planes to a right cone, whose axis would be at right angles to the plane of the ecliptic, and whose semi-angle would be 45°.

That this tendency is too marked to be considered as casual, will be admitted when it is considered that the mean number of inclinations per cent due to each 10° would be 11, while the number of inclinations included between 40° and 50° is nearly 16, being nearly 50 per cent greater than the mean.

To determine, in like manner, whether the nodes and perihelia exhibit any preference for any particular longitude, I have constructed a table giving the number of nodes and the number of perihelia in every 100 included within every 30° of longitude, from 0° to 360°.

From this table it would appear that there is a tendency in the nodes to crowd into the third and seventh signs of the ecliptic. The mean number of nodes due to each sign being 8·33, the number of the third sign is very nearly 12, and the number in the seventh nearly 10; the former being nearly 50 per cent and the latter 20 per cent above the mean.

The perihelia, in the same manner, show a tendency to crowd into the third, fourth, and tenth signs, the numbers in the third and fourth being 12, and the number in the tenth nearly 13, each being about 50 per cent above the mean.

It is difficult to conceive, when the large number of comets upon which these calculations have been based, is considered, that these results, either as regards the perihelia or the nodes, can be casual.

The distribution of the points where the comets arrive at perihelion, with reference to their distances from the sun, supply another ground of classification.

This object may be effected by the formation of a table, exhibiting the number of comets in every 100 which have their perihelia between the surfaces of spheres distributed round the sun, with the radii of 20, 40, 60, &c. millions of miles.

It appears from such a table that only a small proportion of the perihelia lie outside the sphere, whose radius is 120 millions of miles. This circumstance must be ascribed to the fact, that comets moving in such orbits will mostly escape observation; but it may, perhaps, be assumed that, *cæteris paribus*, the comets, whose perihelia lie within a sphere through the earth's orbit, have nearly equal chances of being observed. If this be assumed, then it will follow that the numbers of such comets which have been observed are nearly proportioned to their total numbers; and, therefore, that the numbers within this limit in the preceding table do actually represent approximately the distribution of the points of perihelion round the sun.

If we compare, then, the number of perihelia situate between the equi-distant spheres indicated in the table above referred to, with the cubical spaces through which they are respectively distributed, we shall obtain an approximate estimate of the *density* of their distribution in relation to the distances from the sun. I have computed the following table with this view. In the second column I have given the number of comets per cent, whose perihelia are included between the equi-distant spheres: in the third column, the numbers express the cubical spaces between sphere and sphere, the volume of the sphere, whose radius is 20 millions of miles, being the cubical unit; and in the fourth column the numbers are the quotients of those in the second, divided by those in the third; and,

therefore, express the successive densities of the perihelia between sphere and sphere.

		Numbers of Perihelia.	Cubical Space.	Density of Perihelia.
0 —	20	8·65	1	8·65
20	40	11·70	7	1·67
40	60	20·30	19	1·06
60	80	17·20	37	0·47
80	100	20·80	61	0·34
100	120	8·65	91	0·095

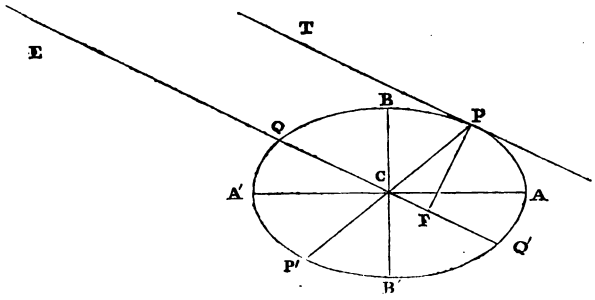
It is evident, then, that the density of the perihelia increases rapidly in approaching the sun. If the numbers in the last column of the table be compared with the inverse powers of the distance, it will be found that this increase of density is more rapid than the inverse distance, but less so than the inverse distance squared.

Two planes being supposed to be drawn, each at an angle of 45° with the plane of the ecliptic, and so that the intersection of one of them with the plane of the ecliptic is directed to the 75° of longitude, and that of the other to the 200° of longitude, there is a tendency of the cometary orbits to crowd towards the directions of these planes, not certainly so decided as to establish a law, but too pronounced to be casual.

Note on the Correction to be applied to the Apparent Ellipticity of a Planet, in consequence of the Elevation of the Earth above the Plane of the Planet's Equator. By Mr. Grant.

As the subject of the ellipticity of the planets, more especially *Saturn*, appears to excite considerable interest at present among astronomers, it may be desirable to exhibit a convenient mode of computing the effect due to the elevation of the earth above the plane of the planet's equator.

Let A B A′ B′ represent an elliptical section of the planet formed



by a plane passing through the polar axis B B′ and the eye of an

observer at the surface of the earth. Let A A′ be the equatoreal diameter situate in this plane, and let C Q E represent the direction of the observer, with respect to C, the centre of the planet. It is manifest that the angle A′ C E will then represent the elevation of the earth above the plane of the planet's equator. Let P P′ represent the extremities of the apparent polar axis of the planet, and let the direction of the visual ray at P be indicated by P T, which will evidently be a tangent at that point to the elliptical section of the planet. Moreover, since the radius C P is small compared with the distance of the earth from the planet, P T may be supposed without sensible error to be parallel to E C, and, consequently, Q Q′ will be a conjugate diameter to P P′.

Draw P F perpendicular to Q Q′. The apparent semi-polar axis will obviously be measured by the angle which this line subtends at the place of the observer on the surface of the earth.

Let $\angle$ A′ C Q $= \varphi$, P F $= p$.

By the well-known property of the ellipse, we have

$$PF = \frac{AC \times BC}{CQ}$$

$$\text{or} \quad p = ab \cdot \frac{\sqrt{1 - e^2 \cos^2 \varphi}}{a\sqrt{1 - e^2}}$$

$$\text{Hence} \quad b = p \cdot \frac{\sqrt{1 - e^2}}{\sqrt{1 - e^2 \cos \varphi}}$$

e being the *eccentricity* of the spheroid.

This expression agrees with Bessel's remark (*Ast. Nach.* vol. xii. p. 169), wherein he states that the reduction of the projection of the spheroid upon its lesser axis may be effected by multiplying the apparent polar diameter by $\frac{\sqrt{1 - e^2}}{\sqrt{1 - e^2 \cos^2 \varphi}}$.

It may, however, perhaps, be found convenient to present this result in the form of a *correction* to the apparent ellipticity of the planet, so as to deduce the true ellipticity from the apparent, and *vice versâ*, independently of a knowledge of the polar diameter.

$$\text{Now} \quad p = \frac{b\sqrt{1 - e^2 \cos^2 \varphi}}{\sqrt{1 - e^2}} = a\sqrt{1 - e^2 \cos^2 \varphi}$$

$$\therefore \quad \frac{p}{a} = \sqrt{1 - e^2}\sqrt{1 + \frac{e^2}{1 - e^2} \cdot \sin^2 \varphi}$$

$$= \sqrt{1 - e^2} + \frac{1}{2} \cdot \frac{e^2}{\sqrt{1 - e^2}} \cdot \sin^2 \varphi - \frac{1}{8} \cdot \frac{e^4}{(1 - e^2)^{\frac{3}{2}}} \cdot \sin^4 \varphi + \&c.$$

$$\text{or} \quad \frac{p}{a} = \frac{b}{a} + \delta,$$

putting $\delta = \frac{1}{2} \cdot \frac{e^2}{\sqrt{1-e^2}} \cdot \sin^2\phi - \frac{1}{8} \cdot \frac{e^4}{(1-e^2)^{\frac{3}{2}}} \sin^4\phi$ + &c.

Hence $$1 - \frac{b}{a} = 1 - \frac{p}{a} + \delta$$

or $$\epsilon = \epsilon' + \delta,$$

where ϵ denotes the *true* ellipticity of the planet, and ϵ' the *apparent* ellipticity.

It appears, then, that δ represents the correction to be applied to the apparent ellipticity of the planet, in order to determine the true ellipticity,

Since $$\frac{p}{a} = \frac{b}{a} + \delta$$

$$\therefore \quad b = p - a\delta$$

In this case then $a\delta$ denotes the correction of the polar semi-diameter.

When the question refers to the actual ellipticity of any planet, the first two terms of δ will be found quite sufficient for all practical purposes. We may, therefore, thus recapitulate the results,—

$$\epsilon' = 1 - \frac{p}{a} \tag{1}$$

$$\delta = \tfrac{1}{2} \cdot \frac{e^2}{\sqrt{1-e^2}} \cdot \sin^2\phi - \tfrac{1}{8} \cdot \frac{e^4}{(1-e^2)^{\frac{3}{2}}} \cdot \sin^4\phi \tag{2}$$

$$\epsilon = \epsilon' + \delta \tag{3}$$

$$b = p - a\delta \tag{4}$$

The last equation will be applicable in the case wherein it is required to deduce from a series of measures of the *apparent* polar diameter made in different parts of the planet's orbit, the corresponding values of the *true* polar diameter with the view of subjecting the results to direct comparison, after they have been reduced to the mean distance from the earth.

The numerical values of the coefficients of $\sin^2\phi$, $\sin^4\phi$, in the expression for δ, may be readily determined in any particular case from any assigned value of the ellipticity. Thus, with respect to *Saturn*, if we assume with Bessel,

$$\epsilon = 0{\cdot}098{,}048$$

we have $$e = \sqrt{2\epsilon - \epsilon^2} = 0{\cdot}431845$$

therefore $$\delta = (9{\cdot}014{,}4440)\sin^2\phi - (7{\cdot}772{,}6766)\sin^4\phi$$

and, consequently,

$$\epsilon = \epsilon' + (9{\cdot}014{,}4440)\sin^2\phi - (7{\cdot}772{,}6766)\sin^4\phi$$

where the numbers within the parentheses represent the logarithms of the coefficients.

The correction will manifestly attain its maximum value when the earth has ascended to its greatest elevation above the equator of the planet. According to Bessel, the inclination of the plane of the ring (and consequently that of the equator of the planet) to the ecliptic, amounts to $28^\circ\ 10'\ 44''{\cdot}7 - 0''{\cdot}350\ (t - 1800)$. In consequence of the latitude of the planet, when it is at a distance of 90° from the node of the ring, the maximum elevation of the earth above the equator of the planet will generally be somewhat less than this quantity. Let us suppose the maximum elevation of the earth to be 27°, we have, then,

$$\delta = 0{\cdot}021{,}055$$

$$\epsilon' = \epsilon - \delta = 0{\cdot}098{,}048 - 0{\cdot}021{,}055$$

$$= 0{\cdot}076{,}993 = \frac{1}{12{\cdot}9882}$$

This is, therefore, the minimum apparent ellipticity which the planet can exhibit, supposing the true ellipticity to be 0·098048.

Next to determine the ellipticity indicated by Mr. De La Rue's measures. It appears from his note that they were made on November 19, 1852. Now, if we calculate the elevation of the earth above the equator of the planet for the same day, by means of Bessel's elements of the ring (*Ast. Nach.* vol. xii., p. 170), we find it to be $20^\circ\ 32'{\cdot}1$. Hence, using the value of e deducible from Bessel's measures, we obtain $\delta = 0{\cdot}012{,}631$, and repeating the process with the new value of e deducible from this result, we obtain for the more accurate value of the correction,

$$\delta = 0{\cdot}012{,}177.$$

Again, the mean value of his measures of the apparent polar diameter was found by him to be $18''{\cdot}68$, and the mean value of the equatoreal diameter to be $20''{\cdot}35$.

Whence $$\epsilon' = 0{\cdot}082{,}064 = \frac{1}{12{\cdot}1856}$$

and $$\epsilon = \epsilon' + \delta = 0{\cdot}082{,}064 + 0{\cdot}012{,}177$$

$$= 0{\cdot}094{,}241 = \frac{1}{10{\cdot}611}$$

The corresponding value of the *apparent* ellipticity deducible from Bessel's measures, is,

$$\epsilon' = \epsilon - \delta = 0{\cdot}098{,}048 - 0{\cdot}012{,}631 = 0{\cdot}085{,}417.$$

The measures from which Mr. Lassell deduced the apparent ellipticity mentioned by him in his letter to Mr. De La Rue, inserted in the *Monthly Notices* for January, viz. $\frac{1}{12{\cdot}697} = 0{\cdot}078\ 764$,

appear to have been made about the same time.* Hence, assuming the same value of ϕ, we obtain $\delta = 0{\cdot}011{,}734$, and therefore,

$$\epsilon = \epsilon' + \delta = 0{\cdot}078{,}764 + 0{\cdot}011{,}734$$
$$= 0{\cdot}090{,}498 = \frac{1}{11{\cdot}050}$$

The corresponding value of ϵ', deducible from Mr. Main's ellipticity, viz. $\frac{1}{9{\cdot}227}$, will be $0{\cdot}09434 = \frac{1}{10{\cdot}6}$; and the value for the same date, indicated by the ellipticity which Mr. Main has derived from the measures made by Bessel during the almost total disappearance of the ring, viz. $\frac{1}{9{\cdot}06}$, will be $0{\cdot}09608 = \frac{1}{10{\cdot}408}$.

As a wish has been expressed to have some standard of comparison in observations of this kind, the following table has been calculated for the latter part of the present year, from the values of the equatoreal and polar diameters of the planet corresponding to the mean distance from the sun ($\log = 0{\cdot}979{,}6480$), as deduced by Bessel from the totality of his measures, viz. $17''{\cdot}053$ and $15''{\cdot}381$ respectively. The second column in the table exhibits the elevation of the earth above the equator of the planet, which is, in the present instance, towards the *south* pole of the planet. The third and fourth columns contain the corresponding *apparent* equatoreal and polar diameters of the planet. The numbers in the fifth column give the correction to be applied to the true ellipticity in order to find the apparent ellipticity, the values of which are exhibited in the sixth and last column. The table commences with the 1st of September, when the planet will have become sufficiently favourable for observation.

Date. 1858.	φ	$2a$	$2b$	δ	ϵ'
	° ′	″	″		
Sept. 1	24 53·8	18·253	16·795	0·01814	0·07991
11	24 53·1	18·590	17·104	0·01812	0·07993
21	24 51·0	18·922	17·409	0·01807	0·07998
Oct. 1	24 47·4	19·237	17·697	0·01799	0·08005
11	24 42·6	19·521	17·956	0·01788	0·08016
21	24 36·7	19·761	18·174	0·01775	0·08030
31	24 29·8	19·944	18·339	0·01760	0·08045
Nov. 10	24 22·3	20·059	18·442	0·01743	0·08062
20	24 14·5	20·098	18·475	0·01726	0·08079
30	24 6·9	20·060	18·436	0·01710	0·08096
Dec. 10	24 0·0	19·946	18·328	0·01694	0·08111
20	23 54·3	19·762	18·157	0·01682	0·08123
30	23 50·2	19·520	17·933	0·01673	0·08132

* Mr. Lassell's measures have been communicated to the Society since the above was written, and are inserted at page 183 of the present number of the *Monthly Notices*.

On Twinkling. By M. Arago.*

M. Arago commences his inquiry by giving an exact definition of the term *scintillation*. It is, he remarks, from not adopting a similar practice that astronomers and other physical inquirers have hitherto failed to arrive at a satisfactory explanation of the phenomenon. He affirms, then, that, in so far as naked-eye observers of the heavens are concerned, scintillation or twinkling consists in very rapid fluctuations in the brightness of the stars. These changes are almost always accompanied by variations of colour and certain secondary effects, which are the immediate consequences of every increase or diminution of brightness; such as considerable alterations in the apparent magnitudes of the stars, and in the length of the diverging rays, which appear to issue in different directions from their centres.

It has been remarked from a very early age that the phenomenon of twinkling is accompanied by a *change of colour*. M. Arago states, upon the authority of M. Babinet, that the name of *Barakesch*, given by the Arabians to the star *Sirius*, signifies *the star of a thousand colours*. He also cites various passages from the works of Tycho Brahé, Kepler, and other modern inquirers, indicative of a similar change of colour attending the process of twinkling.

M. Arago asserts further, that *the twinkling of the planets* is a well-established fact. Thus observers generally, from Tycho Brahé downwards, have remarked that *Mercury* twinkles very strongly. Gassendi even maintains that it is on account of this circumstance that the appellative Στιλβων has been applied to the planet. *Venus* has also been observed to twinkle, and even *Mars* and *Jupiter*, though feebly. Tycho Brahé has remarked that *Saturn* never twinkles; but this opinion has been controverted by Scheiner and other observers, although it is generally admitted that the phenomenon is exceedingly difficult to be recognised in the case of this planet.

An impression has generally prevailed that the stars do not twinkle in telescopes. M. Arago, however, asserts that this opinion is erroneous. Even a few years after the invention of the telescope Simon Marius remarked that, by removing the eye-piece of the telescope, the images of the stars and planets appear enlarged, and exhibit rapid fluctuations of brightness and colour. In 1813, Nicholson showed that a similar phenomenon would be produced if the eye-piece of an achromatic telescope was pushed out of focus. The same inquirer also gave an account of another experiment, which tended to illustrate the rapid fluctuation of colour which the stars undergo during the process of twinkling. The telescope being adjusted to distinct vision, he applied to it a smart vibration, which caused the image of the star to be transformed into a curved line of light, returning into itself, and diversified by

* *Annuaire du Bureau des Longitudes*, 1852, pp. 365–504.

several colours. A similar phenomenon presented itself on the occasion of each successive vibration. He estimates that each colour occupied about a third of the whole length of the curve, and assuming that he applied ten vibrations to the telescope in the course of a second, he hence concluded that the light of *Sirius passes through thirty changes of colour in a second of time.*

It follows, as a necessary consequence of this experiment, that the stars in general shine only by a portion of their light, the effect of twinkling being to diminish their brightness. It is easy to conceive, therefore, that a star which is estimated to be of the seventh magnitude, because it is usually invisible to the naked eye, might become distinctly perceptible if the phenomenon of twinkling were to cease.

After giving a detailed account of the circumstances which accompany the twinkling of the celestial bodies, and of the modifications to which they are subject, M. Arago next proceeds to explain his views of the physical origin of the phenomenon. He maintains that the only satisfactory theory which can be advanced on the subject is that which connects the phenomenon with the principle of the interference of light. He illustrates the latter principle by the well-known experiment, in which two rays of light emanating in different directions from a luminous point, are made to converge again by being reflected from the surfaces of two mirrors, and to combine together, or to destroy each other, according to the conditions of the experiment. If the light from which the rays issue be homogeneous, and if the routes severally traversed by them be made to *differ in length* by gradually displacing one of the mirrors, the point where they meet after reflexion will, in some positions of the movable mirror, exhibit a very vivid light, while in other intermediate positions it will appear *quite black*. The positions of the movable mirror, corresponding to which the two rays thus alternately conspire together, or destroy each other, will vary with the colour of the spectrum employed in the experiment. It results from this important fact, that when rays of white light emanate from the luminous point, they will exhibit at their point of concourse after reflexion a succession of prismatic colours, depending in each case on the position of the movable mirror.

It is found that similar effects may be produced if, instead of causing the routes of the two rays to differ in length, the *refrangibility* of the media through which they pass be subjected to a similar variation.

It is upon these two facts that M. Arago has established his theory of scintillation.

In the case of telescopic observations, he supposes that the rays of light which enter the telescope at opposite extremities of a diameter of the object-glass may have traversed strata of the upper regions of the atmosphere, which, either from variations of density or temperature, or from hygrometric causes, may possess different refractive powers. It might happen from this cause, that the red rays at the one extremity of the diameter might totally destroy

those at the opposite extremity, and that the focus might pass from the normal colour of white to that of *green*, the complimentary colour of red. In the next instant the green might be totally destroyed, and the colour at the focus would, consequently, be red; and similar effects might manifestly be produced each successive instant, by the destruction now of one colour and now of another colour of the spectrum.

Generally, the rays will only *partially* destroy each other by their interference; in which case the light will still be coloured at the focus, although less intensely than if the destruction had been complete.

M. Arago had already established by experiment, that if even the twentieth part of a pencil of light were extinguished by the interference of any of the homogeneous rays, the light at the focus would appear *sensibly* coloured. It would, therefore, be sufficient that the strata of the atmosphere should, by reason of their unequal refrangibility, affect intermittently, and in a suitable degree, the twentieth part of the rays which the surface of a lens embraces in order that the focal point should acquire in succession the different prismatic colours. "Now," says M. Arago, "if we take into consideration the great length of the route traversed by the light from the superior limits of the atmosphere to the object-glass of the telescope; if we reflect, moreover, on the small difference of refrangibility which suffices to occasion the passage of two rays from the state of accord to that of destruction, on the effect of winds, however moderate, bringing incessantly new atmospheric strata before the telescope, it cannot excite any surprise that in observing *Sirius*, a star sufficiently low in our latitudes, as many as thirty changes of colour in a second have been noted." Having thus explained, by the principle of the interference of light, the twinkling of the stars in telescopes, M. Arago finds no difficulty in applying the same explanation to observations with the naked eye.* He then proceeds to show how the twinkling of the planets may be accounted for by the same principle, and he concludes the exposition of his views on this interesting subject by suggesting three different modes of measuring the scintillation of a star. We shall confine ourselves to a brief notice of the first of these *scintillomètres*, as he terms them, which is unquestionably the most ingenious in its conception, and the most conclusive as regards the character of its results. It depends on an experiment, which is originally due to M. Arago himself, and which he first gave an account of in the year 1824.† If a diaphragm be placed before the object-glass of a telescope, so as to allow the light to pass through a circular aperture, and if a

* M. Arago has mainly explained the phenomenon of twinkling, whether as discernible in telescopes or as visible to the naked eye, by the unequal refrangibility of the media traversed by the rays of light which enter the object-glass of the telescope or the eye of the observer; but he has also endeavoured to show that some difference must exist in the *lengths* of the routes traversed by the rays, which would be capable in some degree of accounting for the phenomenon.

† *Annales de Chimie et de Physique*, tome xxvi. p. 431.

star be observed with the telescope when the eye-piece is in the position of distinct vision, the image of the star will resemble a vacillating disc of light surrounded by alternate dark and bright rings. If the eye-piece be now gradually pushed in, there is a second position, in which the luminous disk in the centre will be replaced by a *black hole* surrounded by alternate bright and dark rings. By continuing to push the eye-piece towards the focus a third position will be found, in which the image will resemble that observed in the first instance; and thus a constant recurrence will take place as the eye-piece is pushed forward, the image alternating between a *luminous centre* surrounded by *dark* and bright rings, and a *central dark hole* surrounded by *bright* and dark rings. Now, to determine the second position of the eye-piece, viz. that in which the image of the star exhibits a black hole in its centre, instead of observing the star directly, the eye-piece may be placed exactly midway between the first and third positions. If the telescope be now directed to a star which twinkles, the phenomenon will manifest itself in a succession of accidental reappearances of a luminous point in the dark hole, *and these reappearances will be more numerous as the twinkling is stronger.* At the request of M. Arago, a series of observations of this kind were made by MM. Goujon and C. Matthieu. The following selection will serve as an illustration of them :—

Jan. 14, 1851.

Stars.	Height above the Horizon.	Number of Apparitions in Five Minutes.
Sirius	20°	40
Rigel	31°	17
Aldebaran	57°	13
Capella	81°	8

Jan. 22, 1851.

Sirius	24°	28
Procyon	46°	20
α Orionis	48°	15
Pollux	69°	12

M. Arago had concluded from theoretical considerations, that if the eye-piece were shifted from the second position to some position intermediate between the second and third, the apparitions of the luminous point would be more frequent than in the first instance. The results of actual observations afforded a complete confirmation of his conclusion.

Having fully expounded his own view of the origin of twinkling, M. Arago next examines the various theories advanced by preceding writers on the subject, and points out their inadequacy in every case to account for the phenomena.

M. Arago states that although he feels convinced that, in connecting the phenomenon of scintillation with the principle of interferences, he has viewed the subject in its proper aspect, still he is

far from being of opinion that nothing further remains to be done. "For example," says he, "no one of my acquaintance has connected in an entirely satisfactory manner, and even to their numerical values, the planetary disks which the stars acquire, and the rings which surround them, with the theory of interferences." He adds, that he had been informed that M. Schwerd, a geometer of Germany, had succeeded in this research; but he was told at the same time that, according to the calculations of M. Schwerd, which besides were applied exclusively to simple lenses not achromatic, the diameters of the planets when seen in a telescope with a reduced aperture ought to appear enlarged like the stars, a result entirely contradicted by observation.*

Exposé historique des Travaux exécutés jusqu'à la fin de l'année 1851, *pour la Mesure de l'Arc du Méridien entre Fuglenæs,* 70° 40′, *et Ismäil,* 45° 20′. *Publié au Nom de l'Académie Impériale des Sciences de St. Pétersbourg, par W. Struve, Directeur de l'Observatoire Central de Russie. Suivi de Deux Rapports de M. G. Lindhagen, Astronome de l'Observatoire central sur l'Expédition de Finmarken, faites par Ordre du Gouvernement Norvégien en* 1850; *et sur les Opérations de Lapponie, exécutées en* 1851, *sous les Auspices de l'Académie Royale des Sciences de Stockholm.* 4to. pp. 44. St. Pétersbourg, 1852.

In this Report M. Struve gives a brief history of the operations connected with the measurement of the Russo-Scandinavian arc of the Meridian, which has now attained the colossal extent of 25° 20′. This great undertaking was first commenced in Russia in the year 1816, and was finally brought to a close in 1851, the labours of its execution having, consequently, embraced a period of thirty-seven years.

As has happened in some other similar instances, the measurement of this arc was not an independent project undertaken with a view to the advancement of physical science, but was suggested by the operations connected with the trigonometrical survey of a portion of the Russian empire. In 1816, M. Struve and General Tenner were charged, the one with the trigonometrical survey of Livonia and the other with that of Lithuania; and it occurred to both geometers to profit by their labours in measuring an arc of the meridian in their respective provinces.

* In the *Cambridge Philosophical Transactions*, vol. v. p. 283 *et seq.*, Mr. Airy, proceeding upon the principles of the undulatory theory of light, has investigated the diffraction produced by an object-glass having a reduced aperture of a circular form, and has calculated the numerical values of the diameters of the spurious disks of the stars and of the alternate dark and bright rings surrounding them. The results were found to present a satisfactory agreement with Fraunhofer's measures.—Editor.

It has been established by numerous experiments made with the pendulum in different parts of the world, that the parallels on the surface of the earth are nearly circles; or in other words, that the earth, supposing its surface reduced to the level of the ocean, may be regarded as a solid of revolution. From this circumstance it follows, that we may investigate the dimensions and figure of the earth by combining together the partial arcs measured under different longitudes. The solution of the problem would not, therefore, present any serious difficulty, were it not that, independently of the unavoidable small errors of observation, there exists a disturbing influence, which generally affects the comparison of two arcs of the meridian measured under very different latitudes. This consists in a deviation of the plumb-line, occasioned either by the inequalities of the surface of the earth *above* the level of the ocean, or by irregularities in the densities of the strata *beneath* the same level, which tends in a greater or less degree to vitiate the result of every measure of an arc of the meridian. According to the researches of the illustrious Bessel, the probable error of a determination of latitude due to this cause amounts to $1''{\cdot}8$. Hence it is manifest, that the accuracy of the final results in any case will be enhanced by selecting for the field of operations a region subject as little as possible to local disturbance, and by giving a considerable extension to the arc measured.

The region through which the Russian arc extends appears to offer peculiar advantages for geodesical operations, in so far as the effects due to inequalities on the surface of the earth are concerned. Throughout the whole of this region, extending from the Danube to Lapland, and comprising an arc of the meridian of 20°, there does not exist a single mountainous mass tending to produce a sensible deviation of the plumb-line. In the southern portion, indeed, between the Dniester and the Dwina, the surface is so level and so wooded that it was frequently necessary to have recourse to artificial stations, in order to obtain a clear view of the surrounding region.

M. Struve having completed the survey of Livonia in the year 1819, suggested to the University of Dorpat the desirableness of profiting by the stations of the survey, to measure the arc of the meridian of $3^\circ\ 35'$, comprised between the island of Hogland in the Gulf of Finland, and the town of Jacobstadt in Courland,—an arc passing very nearly through the meridian of the observatory of Dorpat. Through the favourable representations of that body, the sanction of the Emperor was obtained for the execution of the project, and the operations connected with it were finally completed in the year 1827. In the same year General Tenner completed the measurement of an arc of $4^\circ\ 32'$, between Bristen in Courland, and Belin in the province of Grodno, by taking advantage of the principal triangles of his operations situate in the prolongation of the Observatory of Wilna.

Bristen, the northern point of the Lithuanian arc, being situate only $4'\ 47''$ to the south of Jacobstadt, the extreme southern station

of the arc of Livonia, it obviously became desirable to unite the two partial arcs together into one complete arc. This was effected, independently, by M. Struve and General Tenner, in the years 1827–28. The results of the two geometers, accompanied by sealed letters, were submitted to the examination of the illustrious Bessel, who found them to present a most satisfactory accordance; and they were finally published in the year 1832. Thus was completed the first period in the history of the Russian arc, which at this stage of its progress extended from Hogland to Belin, and embraced an amplitude of 8° 2′·5.

The Russian Arc of the Meridian thus measured by M. Struve and General Tenner was, with two exceptions, the most extensive hitherto determined, ranking immediately after the great arc of India and the French arc. It constituted one of the ten arcs of the meridian which Bessel employed in his famous investigation of the dimensions and figure of the earth.

M. Struve did not fail soon to perceive the desirableness of extending the arc northwards, so as to form a junction with the Swedish arc measured in Lapland. Two circumstances suggested the importance of executing this operation. In the first place, it appeared that the northern arcs were not represented in sufficient fulness in Bessel's researches, although the coefficient of the ellipticity mainly depends on a comparison of such data with the arcs near the equator. Secondly, there existed some doubts respecting the Lapland arc, which tended to impair its trustworthiness as one of the elements for determining the earth's figure. This arc, as is well known, was originally measured by the French Academicians and Celsius in the year 1735; but, upon being re-measured and somewhat extended in the beginning of the present century, under the auspices of the Academy of Sciences of Stockholm, it was found that the results of the two operations assigned considerably different lengths to a degree of the meridian under the polar circle.

In 1830, M. Struve obtained the consent of the Emperor for the prolongation of the Russian arc through Finland to Tornea, the southern extremity of the Lapland arc. This undertaking was forthwith commenced, and was ultimately completed in the year 1845, M. Struve having for his principal coadjutor M. Wolstedt, now Director of the Observatory of Helsingfors, who continued during ten years to execute all the astronomical observations and all the operations connected with the measurement of the terrestrial angles, although physical obstacles of the most formidable nature constantly opposed his efforts. The length of the arc measured on this occasion was 5° 26′.

During the execution of this operation, General Tenner, by prosecuting his geodesical operations, was enabled to prolong the arc southwards to the river Dniester, which forms the northern frontier of the province of Bessarabia. This gave a further addition of 3° 18′ to the Russian arc, which now extended from Tornea, lat. 65° 50′ 34″, to Souprounkovtsi, lat. 48° 45′ 1″, and attained an amplitude of 17° 5′ 33″. In 1847, M. Struve communicated a note to the Aca-

demy of Sciences of St. Petersburg, containing the provisional results deducible from this arc, in which he announced that they seemed to indicate an enlargement of the value assigned by Bessel to the terrestrial ellipticity, viz. $\frac{1}{299{\cdot}15}$.

While the extension of the Russian arc in both directions, to which allusion has just been made, was in the course of being executed, the Imperial Government gave its sanction that the operations should be prolonged southwards as far as the River Danube, through the entire province of Bessarabia, by which an additional length of 3° 25′ would be given to the arc. This project was carried into complete effect in the year 1849, when the Russian arc finally attained a length of 20° 31′, extending from Tornea in Lapland, to Ismäil on the Danube.

The standard of linear dimensions employed in the measurement of the Russian arc had been compared with the toise of Peru by M. Arago, in the year 1821. As it was desirable that a similar comparison should be instituted between it and the standard employed in the measurement of the great arc of India, M. Struve proceeded to England in 1847, with a view to the promotion of this object. Of the two standards employed in this operation, one which Colonel Everest had brought from India was, with the sanction of the East India Company, transported to Russia, and compared directly with the standard of Pulkowa.*

In 1844, M. Struve was appointed by the Academy of Sciences of St. Petersburg to proceed to Stockholm, with the view of suggesting to the Swedish Government the expediency of prolonging the Russian arc to the Northern Ocean. This mission was attended with complete success. The measurement of the Norwegian portion of the arc was assigned to M. Hansteen of Christiania, and that of the Swedish portion to M. Selander, Director of the Observatory of Stockholm. The actual execution of the operations connected with the Norwegian arc is due to M. Klouman and M. Lindhagen, the latter of whom was detached from the Observatory of Pulkowa to assist in the undertaking. The measurement of the entire Scandinavian arc was finally completed in 1851. The Swedish arc extends from Tornea, lat. 65° 51′, to Atjick, lat. 68° 54′, and has an amplitude of 3° 3′; the Norwegian arc extends from Atjick to Fuglenaes, lat. 70° 40′, and has an amplitude of 1° 46′. The two arcs combined together have, therefore, an amplitude of 4° 49′. "It is thus," says M. Struve, "that the operations of the Scandinavian arc, commenced in 1846, have been completed in 1851. During these six consecutive years, M. Selander, associated with several assistants, has annually undergone the fatigues and perils attending a geodesical and astronomical operation, to be executed in an almost desert country, and situate between 66° and 69° of latitude. We sincerely congratulate M. Selander on the

* The Indian standard was brought back to this country in the year 1850 by M. Otto Struve, and was deposited in the hands of the Astronomer Royal, agreeably to the engagement entered into with the East India Company.

accomplishment of his arduous task; and certainly the learned world will agree with us in awarding to him the acknowledgments which are due to the execution of a scientific work of no less importance than difficulty."

The Russian and the Scandinavian arcs of the meridian now form one continuous arc,—appropriately termed by M. Struve the Russo-Scandinavian arc,—extending from Ismäil on the Danube, lat. 45° 20′, to Fuglenaes on the Northern Ocean, lat. 70° 40′, and possessing an amplitude of 25° 20′. The scientific world cannot fail to look forward with much interest to the results relative to the ellipticity of the earth deducible from the data furnished by this colossal undertaking.

Anleitung zur Geographischen Ortsbestimmung vorzüglich Mittels des Spiegelsextanten, von Mag. J. G. F. Bohnenberger. Neu Bearbeitet von Dr. G. A. Jahn, mit fünf tafeln Abbildungen. Göttingen, 8vo. 1852.

In this new edition of a work, which has long enjoyed a well-merited popularity in Germany, several improvements are effected, which are calculated to adapt the work to the present advanced state of astronomical and geographical science. The portion in the original edition relating to the quadrant is entirely suppressed, and new subjects introduced in its stead; among which may be mentioned an explanation of the use of the patent reflecting circle of Pistor and Martins of Berlin, and of the portable transit instrument for the determination of latitudes by means of observations made in the prime vertical.

COMET (I.) 1853.

Observations of this comet, made in various places, will be found in No. 856 of the *Astronomische Nachrichten*.

COMET (II.) 1853.

The comet discovered by M. Schweizer on the 4th of April has been observed at Hamburgh by M. Rümker. It appeared tolerably distinct, with a nucleus and short tail. The following observation of it is given by M. Rümker, in No. 857 of the *Astronomische Nachrichten*:—

1853.	Hamb. M.T. h m s	Comet's R.A. ° ′ ″	Decl. ° ′ ″
April 16	13 43 48	306 55 48·9	+ 12 33 10·9

Apparent Place of Star of Comparison, *η Delphini*.

R.A. h m s	Decl. ° ′ ″
20 26 59·48	+ 12 31 23·2

COMET (II.) 1852.

Observations of this comet, made at the observatory of Berlin, and extending to Jan. 9, 1853, are given in the *Astronomische Nachrichten*, No. 856, p. 250.

PSYCHE.

An ephemeris of this planet for the opposition of 1853 has been calculated by Dr. Oudemans of Leyden (*Ast. Nach.* No. 858, p. 294). It extends from April 12 to May 12.

COMET OF ENCKE.

Observations of this comet, made at the observatory of Breslau in the year 1852, will be found in No. 858 of the *Astronomische Nachrichten*.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIII. *May* 13, 1853. No. 7.

JOHN LEE, Esq. LL.D. in the Chair.

Thomas William Burr, Esq., of Highbury,
was balloted for and duly elected a Fellow of the Society.

It may be as well to remind Fellows of the Society, that the pages of the *Monthly Notices* are open for the publication of all brief communications relating to astronomical science, except so far as regards planetary observations and ephemerides, the necessity for inserting which in this periodical is generally superseded by the earlier and more complete publication of intelligence of a similar kind in the *Astronomische Nachrichten*. It is manifest, however, that a few observed positions of any newly-discovered body, whether planet or comet, as well as a determination of the elements of its orbit, will serve to enhance the usefulness of the *Monthly Notices*; and it is scarcely necessary to add, that any contributions of this nature will be inserted with pleasure.

On a new Solution of the Problem of Planetary Perturbation.
By Professor Encke.

(*Extract of a Letter to the Astronomer Royal.*)

"The subject of the small planets has continued to occupy my attention so much, that I could not communicate with you until I had relieved my mind of this load of anxiety. I consider myself very fortunate on this account in being enabled, through the meritorious services of my excellent Dr. Brünnow, to transmit to you in the annexed paper the mean elements of *Flora*, with the perturbations of the same planet as produced by *Jupiter* and *Saturn*.

From the mode in which the calculation of these elements has been worked out, I am induced to entertain a confident hope that we have it now in our power to compute the perturbations of the first power of the masses for all the small planets, not even excepting *Pallas*, and that the time bestowed on the calculations is not immoderately long.

"The perturbations produced by *Jupiter* upon *Flora*, which possesses the advantage of being pretty distant from the disturbing planet, but which, on the other hand, has an excentricity of 9° and an inclination of 6°, will admit of being computed in four weeks without any immoderate haste; and even with respect to those of *Pallas*, I would pledge myself, without hesitation, to compute them for *Jupiter* and *Saturn* in the course of a winter, if nothing unusual interfered with the discharge of my ordinary duties.

"On a former occasion I had the honour of transmitting to you a copy of a paper which I communicated to the Academy, and in which I proposed an indirect method. This was not suitable to the object in view, as any one may easily convince himself, since the approximations conducted to the required result only in a few rare cases. Still the proposed method has not been useless, since it has given me the form which is most convenient for calculation, of the integral of the assigned differential equation which always reappears in the problem. By a mere simple consideration of the combinations which present themselves in this inquiry, the following solution has been obtained.

"If the differential equation has the form

$$\frac{d\,d\xi}{dt^2} + \frac{k^2}{r^{03}}\xi = \Sigma\left(a_i^{i'} \cos\left(i\,\mathrm{M} - i'\,\mathrm{M}'\right)\right),$$

in which the cosine may be exchanged for the sine, and M, M', denote the mean anomalies of the disturbed and disturbing planets, the mean motions of which are represented by μ and μ'; then, in every case, will the co-efficient of cos $(i\,\mathrm{M} - i'\,\mathrm{M}')$ or of sin $(i\,\mathrm{M} - i'\,\mathrm{M}')$ be thus determined by integration:—

Let
$$x^0 = r^0 \cos v^0 = p_0 + \Sigma\,(p_m \cos m\,\mathrm{M})$$
$$y^0 = r^0 \sin v^0 = \Sigma\,(q_m \sin m\,\mathrm{M}).$$

Also let
$$\frac{1}{r\mu - i'\mu'} - \frac{1}{s\mu - i'\mu'} = [r, s];$$

then the coefficient of cos $(i\,\mathrm{M} - i'\,\mathrm{M}')$ in the integral will have the following form:—

$$\frac{a_{i-3}^{i'}}{k\sqrt{p}}\left\{[i-1, i-2]\frac{p_1 q_2 - p_2 q_1}{4} + [i, i-3]\frac{p_0 q_3}{2} + [i+1, i-4]\frac{p_1 q_4 + p_4 q_1}{4}\right.$$
$$\left. + [i+2, i-5]\frac{p_2 q_3 + p_3 q_2}{4} + [i+3, i-6]\frac{p_3 q_6 + p_6 q_3}{4} \ldots\ldots\right\}$$

$$+\frac{a_{i-2}^{i'}}{k\sqrt{p}}\left\{[i,i-2]\frac{p_0q_2}{2}+[i+1,i-3]\frac{p_1q_2+p_2q_1}{4}+[i+2,i-4]\frac{p_2q_4+p_4q_2}{4}\right.$$
$$\left.+[i+3,i-5]\frac{p_3q_5+p_5q_3}{4}\ldots\ldots\right\}$$

$$+\frac{a_{i-1}^{i'}}{k\sqrt{p}}\left\{[i,i-1]\frac{p_0q_1}{2}+[i+1,i-2]\frac{p_1q_2+p_2q_1}{4}+[i+2,i-3]\frac{p_2q_3+p_3q_2}{4}\right.$$
$$\left.+[i+3,i-4]\frac{p_3q_4+p_4q_3}{4}+[i+4,i-5]\frac{p_4q_5+p_5q_4}{4}\ldots\ldots\right\}$$

$$+\frac{a_{i}^{i'}}{k\sqrt{p}}\left\{[i+1,i-1]\frac{p_1q_1}{2}+[i+2,i-2]\frac{p_2q_2}{2}+[i+3,i-3]\frac{p_3q_3}{2}\right.$$
$$\left.+[i+4,i-4]\frac{p_4q_4}{4}\ldots\ldots\right\}$$

$$+\frac{a_{i+1}^{i'}}{k\sqrt{p}}\left\{[i+1,i]\frac{p_0q_1}{2}+[i+2,i-1]\frac{p_1q_2+p_2q_1}{4}+[i+3,i-2]\frac{p_2q_3+q_3p_2}{4}\right.$$
$$\left.+[i+4,i-3]\frac{p_3q_4+p_4q_3}{4}+[i+5,i-4]\frac{p_4q_5+p_5q_4}{4}\ldots\ldots\right\}$$

$$+\frac{a_{i+2}^{i'}}{k\sqrt{p}}\left\{[i+2,i]\frac{p_0q_2}{2}+[i+3,i-1]\frac{p_1q_3+p_3q_1}{4}+[i+4,i-2]\frac{p_2q_4+p_4q_2}{4}\right.$$
$$\left.+[i+5,i-3]\frac{p_3q_6+p_6q_3}{4}\ldots\ldots\right\}$$

$$+\frac{a_{i+3}^{i'}}{k\sqrt{p}}\left\{[i+2,i+1]\frac{p_1q_2-p_2q_1}{4}+[i+3,i]\frac{p_0q_3}{2}+[i+4,i-1]\frac{p_1q_4+p_4q_1}{4}\right.$$
$$\left.+[i+5,i-2]\frac{p_2q_5+p_5q_2}{4}+[i+6,i-3]\frac{p_3q_6+p_6q_3}{4}\ldots\ldots\right\}$$
$$\vdots\qquad\qquad\vdots\qquad\qquad\vdots$$

"This expression proceeds in an infinite series of terms, both in the vertical and horizontal directions. It, however, converges very quickly for any given values of i and i', so that in general, only a very small number of coefficients require to be taken into consideration in order to determine the coefficient of the integral; and since, for the sine and cosine, the expression is merely a numerical quantity, when μ and μ' are given, so is the calculation for the very small number of values of i, when i' is constant, by no means difficult.

"Dr. Brünnow has succeeded in avoiding the indirect solution by adding to the equation relative to the undisturbed orbit, viz.:

$$\tfrac{1}{2}\frac{d^2(r_0^2)}{dt^2}=k^2\left(\frac{1}{r_0}-\frac{1}{a}\right);$$

the equation for the disturbed orbit, viz.:

$$\tfrac{1}{2}\frac{d^2(r^2)}{dt^2} = k^2\left(\frac{1}{r}-\frac{1}{a}\right) + x\mathrm{X} + y\mathrm{Y} + z\mathrm{Z} + 2\int\left(\mathrm{X}\frac{dx}{dt} + \mathrm{Y}\cdot\frac{dy}{dt} + \mathrm{Z}\cdot\frac{dz}{dt}\right);$$

Whence, by comparison, there result the following rigorous equations:—

$$\tfrac{1}{2}\frac{d^2(r^2-r^{0\,2})}{dt^2} - \left(\frac{k^2}{r}-\frac{k^2}{r^0}\right) = x\mathrm{X} + y\mathrm{Y} + z\mathrm{Z} + 2\int(\mathrm{X}\,dx + \mathrm{Y}\,dy + \mathrm{Z}\,dz),$$

$$\frac{d^2(x-x^0)}{dt^2} + \frac{k^2x}{r^3} - \frac{k^2x^0}{r^{0\,3}} = \mathrm{X}$$

$$\frac{d^2(y-y^0)}{dt^2} + \frac{k^2y}{r^3} - \frac{k^2y^0}{r^{0\,3}} = \mathrm{Y}$$

$$\frac{d^2(z-z^0)}{dt^2} + \frac{k^2z}{r^3} - \frac{k^2z^0}{r^{0\,3}} = \mathrm{Z}.$$

Let us now suppose

$$r = r^0 + \delta r$$
$$x = x^0 + \xi$$
$$y = y^0 + \eta$$
$$z = z^0 + \zeta,$$

and let us collect together the squares and products of δr, ξ, η and ζ. We hence deduce these four equations:—

$$\frac{d\,d(r^0\delta r)}{dt^2} + \frac{k^2}{r^{0\,3}}(r^0\delta r) = x\mathrm{X} + y\mathrm{Y} + z\mathrm{Z} + 2\int\left(\mathrm{X}\frac{dx}{dt} + \mathrm{Y}\frac{dy}{dt} + \mathrm{Z}\frac{dz}{dt}\right)$$
$$- \tfrac{1}{2}\frac{d\,d(\delta r)^2}{dt^2} + \frac{k^2}{r^{0\,3}}(\delta r)^2 - \frac{k^2}{r^{0\,4}}(\delta r)^3$$

$$\frac{d\,d\xi}{dt^2} + \frac{k^2}{r^{0\,3}}\xi = \mathrm{X} + 3\frac{k^2x^0}{r^{0\,5}}(r^0\delta r) - 6\frac{k^2x^0}{r^{0\,7}}(r^0\delta r)^2$$
$$+ 3\frac{k^2\xi}{r^{0\,5}}(r^0\delta r)$$

$$\frac{d\,d\eta}{dt^2} + \frac{k^2}{r^{0\,3}}\eta = \mathrm{Y} + 3\frac{k^2y^0}{r^{0\,5}}(r^0\delta r) - 6\frac{k^2y^0}{r^{0\,7}}(r^0\delta r)^2$$
$$+ 3\frac{k^2\eta}{r^{0\,5}}(r^0\delta r)$$

$$\frac{d\,d\zeta}{dt^2} + \frac{k^2}{r^{0\,3}}\zeta = \mathrm{Z} + 3\frac{k^2z^0}{r^{0\,5}}(r^0\delta r) - 6\frac{k^2z^0}{r^{0\,7}}(r^0\delta r)^2$$
$$+ 3\frac{k^2\zeta}{r^{0\,5}}(r^0\delta r)$$

"The first of these equations gives $r^0 \delta r$, and this substituted in the others gives ξ, η, ζ.

"These are essentially the first equations of the *Mécanique Céleste*."

"A second step due to Dr. Brünnow is that he has executed the numerical calculations of all the multiplications of the prescribed infinite series. This multiplication admits of being made with convenience and accuracy; and strictly affords the advantage of being far shorter than the substitutions would be if the numerical values for c were determined by multiplications executed analytically. M. Hansen had already recommended this mode of executing the numerical calculation without having further recourse to any analytical process; but it has not, so far as I know, been so generally applied in practice.

"These are the fundamental principles of the solution. Dr. Brünnow, however, has not executed the calculations absolutely in this form, but has introduced the polar co-ordinates r^0 and u^0, conformably to his method of treating the subject of special perturbations. It is quite possible, since the solution of the equations is thereby considerably simplified ($r^0 \delta r$ being already obtained by the first equation, and consequently there only remaining δu and z to be determined), that the abbreviation is so considerable as to outweigh the disadvantages attending the somewhat less elegant symmetry of the final equations. In the meanwhile, being curious to know to what extent the expressions for ξ, η, ζ, would be found to be less convergent than δu, I have also begun the calculation by the formula here given, and hope to be soon prepared with it; should the difference be not too great, the simple mode of deducing the formulæ for ξ, η, and ζ, will always captivate me. However, this is a mere matter of taste.

"We have both found the expression for the disturbing force by the actual developement of $\frac{1}{\varrho^3}$ (ϱ being the distance between the two planets), and then by multiplication have deduced the values of the resolved forces. We thereby derive the advantage of having fewer values of ϱ^3 to calculate throughout the entire extent of the periphery. Perhaps it would be better to develope $\frac{1}{\varrho}$ into a series according to powers of M and M', and then by two multiplications to find $\frac{1}{\varrho^3}$.

"The terms of the second order admit of being easily obtained according to this method by means of Taylor's *Theorem*, since the quantities x, y, z, are only involved, the perturbations of which are already known. But this process of calculation, which in every case is very troublesome, would be useless when the question refers to the small planets, since the elements of these bodies are not known with sufficient accuracy. Dr. Brünnow, by means of his osculating elements, has obtained pretty accurate values of the disturbing forces; and by applying the resulting

perturbations has deduced the mean elements of the planet. To me it is as good as certain that the perturbations thus calculated (the hundredths of seconds are omitted) are sufficiently accurate to be employed next year for a more exact determination of the mean elements. The terms of the first power of the masses which are here given will therefore subserve the object in view, and on a future occasion the question may be investigated, whether the terms of the second order exercise any sensible influence.

"The perturbational values are all given as they follow from the calculations. Moreover, the secular equations of the elements which might be deduced from the terms involving t (the unit of t is the Julian year) are not deduced, since for the next year the calculation in its present form is more convenient. By the way, I see that I have not made any mention of these terms in speaking of the integration of the differential equations. However, what I have said above will be quite sufficient to give you an idea of this method of computing the perturbations.

"*Berlin, April* 24, 1853."

The following are the results obtained by Dr. Brünnow relative to the planet *Flora*, to which Professor Encke alludes at the beginning of the foregoing extract:—

Mean Elements of Flora with the Perturbational Equations for Jupiter and Saturn. Calculated by Dr. Brünnow, Assistant at the Berlin Observatory, April 1853.

For the five normal places which were employed in the special perturbations, by means of the resulting perturbational equations for *Jupiter* and *Saturn*, purely elliptic mean elements were found. In this manner were obtained:—

Epoch 1848, Jan. 1·0, Mean Berlin Time.

	°	′	″	
Mean Anomaly	35	54	3·62	
Longitude of the Perihelion	32	54	28·34	Mean Equinox of the Epoch.
Long. Asc. Node	110	17	48·62	
Inclination	5	53	7·96	
Angle of Excentricity	9	0	56·29	
Mean, Dail. Sid. Mo.	1086″·330982			
Log. Sem. Maj.-axis	0·3426963			

These elements gave the mean place, and by applying the perturbational equations to each of the five normal places, the latter were thus represented:—

		Theor. − Obs. $\Delta\alpha$	$\Delta\delta$
		"	"
1848	Jan. 1·0	− 0·1	+ 1·2
	Apr. 30·0	− 0·2	− 0·6
1849	May 5·5	+ 0·3	+ 1·5
1850	Sep. 18·5	− 0·9	+ 1·9
1852	Mar. 29·0	+ 0·9	+ 1·6

The opposition for 1853 was calculated from the special perturbations. Now, if we calculate it from the purely elliptic elements, taking into account the perturbations, we have:—

		α			δ			
		°	′	"	°	′	"	
1853	July 11·5	311	49	42·0	− 19	47	35·8	By Special Perturbations.
				45·3			32·1	By General ——

The perturbations of $r^0\,\delta r$ are given in seconds. They will require to be changed into units of decimals. z is the co-ordinate of the disturbed planet drawn perpendicular to the plane of the mean orbit. It is given in seconds.

δv is the correction of the true anomaly.

Perturbations of Flora by Jupiter.

	$r\,\delta r$.			δv.		
	cos		sin	cos		sin
	$+47''\cdot3 - 0''\cdot3407\,t$			$-36''\cdot700\,t$		
M	$-12°\cdot0$	$+1°\cdot438\,t$	$-46\cdot577\,t$	$-0''\cdot3$	$-19''\cdot218\,t$	$+3\cdot2$ $-0\cdot592\,t$
2M	$-1\cdot1$	$+0\cdot112\,t$	$-3\cdot627\,t$	$+0\cdot0$	$-3\cdot738\,t$	$+1\cdot3$ $-0\cdot115\,t$
3M	$0\cdot0$	$+0\cdot013\,t$	$-0\cdot423\,t$	$0\cdot0$	$-0\cdot756\,t$	$+0\cdot4$ $-0\cdot023\,t$
4M	$0\cdot0$	$+0\cdot002\,t$	$-0\cdot058\,t$	$0\cdot0$	$-0\cdot155\,t$	$0\cdot0$ $-0\cdot005\,t$

	z.			
	cos		sin	
	$-1''\cdot7 + 0''\cdot202\,t$			
M	$+1''\cdot5$	$+0''\cdot852\,t$	$-0''\cdot2$	$+6''\cdot038\,t$
2M	$+0\cdot3$	$+0\cdot066\,t$	$0\cdot0$	$+0\cdot470\,t$
3M	$0\cdot0$		$0\cdot0$	$+0\cdot055\,t$

Perturbations of Flora by Jupiter—(continued).

	$r\delta r$.		δv.		z.	
	cos	sin	cos	sin	cos	sin
$-M-M'$	− 3″·7	+ 4″·3	− 2″·1	− 2″·2	+1″·7	−0″·7
$-M'$	− 49·5	+ 22·9	− 11·3	− 16·6	+4·3	−1·2
$M-M'$	+158·4	− 66·4	− 35·4	− 85·0	−4·4	+2·0
$2M-M'$	+ 23·6	− 10·4	− 7·3	− 17·3	−1·7	+0·9
$3M-M'$	+ 2·5	− 1·1	− 1·5	− 3·5		
$-M-2M'$	+ 3·6	− 3·8	+ 2·7	+ 2·6		
$-2M'$	+ 54·5	− 54·7	+ 14·0	+ 13·3	−3·2	+2·6
$M-2M'$	−100·6	+101·9	+108·2	+110·2	+6·2	−4·1
$2M-2M'$	−214·7	+198·8	+ 75·4	+ 80·8	+1·5	−1·3
$3M-2M'$	− 16·4	+ 14·8	+ 15·8	+ 17·0	−0·1	+0·1
$4M-2M'$	− 1·5	+ 1·3	+ 3·2	+ 3·5		
$-M-3M'$	+ 1·1	+ 0·2	0·0	+ 1·0		
$-3M'$	+ 17·9	+ 2·6	− 0·9	+ 5·9	−0·2	−0·6
$M-3M'$	− 21·2	+ 20·8	− 6·9	+ 42·2	+1·1	−0·8
$2M-3M'$	− 17·5	−112·4	− 39·8	+ 9·6	−4·3	+6·7
$3M-3M'$	− 15·7	+ 20·6	− 0·3	+ 5·7	−0·3	+0·3
$4M-3M'$	− 1·7	+ 1·8	+ 0·3	+ 1·4		
$-4M'$	− 1·3	+ 0·7	− 0·3	− 0·6		
$M-4M'$	+ 3·1	+ 3·2	− 2·0	− 4·6	−0·2	−0·4
$2M-4M'$	− 15·5	− 10·1	− 5·1	+ 7·3	+1·0	+1·6
$3M-4M'$	− 4·9	− 9·4	− 3·0	+ 2·6	0·0	+0·6
$4M-4M'$	− 1·1	+ 5·9	+ 1·0	+ 0·8		
$5M-4M'$	− 0·2	+ 0·9	+ 0·4	0·0		
$3M-5M'$	+ 1·8	0·0	0·0	− 0·5		
$4M-5M'$	− 1·2	− 1·5	− 0·4	+ 0·2		
$5M-5M'$	+ 0·3	+ 1·4	+ 0·3	0·0		

M denotes the mean anomaly of *Flora*, and M′ the mean anomaly of *Jupiter*.

Perturbations of Flora by Saturn.

	$r^0 \delta r$.		δv.	
	cos	sin	cos	sin
	$+1''.96 \quad +0''.0181\,t$		$1''.520\,t$	
M	$-0''.5 \quad -0''.038\,t$	$0''.0 \quad -0''.977\,t$	$0''.0 \quad -0''.403\,t$	$-0''.0 \quad +0''.013\,t$
2 M	$0\ .0 \quad -0\ .003\,t$	$0\ .0 \quad -0\ .076\,t$	$0\ .0 \quad -0\ .078\,t$	$0\ .0 \quad +0\ .003\,t$
3 M	.....	$0\ .0 \quad -0\ .009\,t$	$0\ .0 \quad -0\ .016\,t$	.....

	z.	
	cos	sin
	$+0''.028\,t$	
M	$0''.0 \quad -0''.116\,t$	$0''.0 \quad -0''.106\,t$
2 M	$0\ .0 \quad -0\ .009\,t$	$0\ .0 \quad -0\ .008\,t$

	$r^0 \delta r$.		δv.		z.	
	cos	sin	cos	sin	cos	sin
$-M-M'$,	$+0''.1$	$-0''.3$	$''$	$''$	$''$	$''$
M',	-0.7	-2.1	$+1.3$	$+0.3$		
$M-M'$,	$+3.3$	$+7.8$	$+3.6$	-1.5	0.0	$+0.1$
$2M-M'$,	$+0.3$	$+0.8$	$+0.7$	-0.3		
$3M-M'$,	.....	.....	$+0.1$	0.0		
$-2M'$,	-1.5	$+3.0$	-0.4	-0.1	$+0.1$	0.0
$M-2M'$,	$+5.4$	-10.0	-5.3	-3.0	$+0.4$	-0.1
$2M-2M'$,	$+2.7$	-5.8	-2.4	-1.2		
$3M-2M'$,	$+0.2$	-0.4	-0.5	-0.3		
$4M-2M'$,	.....	.....	-0.1	0.0		
$-3M'$,	0.0	$+0.5$	-0.1	0.0		
$M-3M'$,	$+0.2$	-1.5	-1.0	-0.1		
$2M-3M'$,	-0.4	-1.2	-0.5	$+0.1$		
$3M-3M'$,	$+0.4$	-0.1	-0.1	-0.1		

M denotes the mean anomaly of *Flora*, and M′, the mean anomaly of *Saturn*. The unit of time is the Julian year of $365\frac{1}{4}$ days.

Extract of a Letter from Sig. Secchi to Captain R. H. Manners, R. N.

" On the occasion of the last observations which I have made of the solar spots, I have read again what Mr. Grant states, in his *History of Physical Astronomy*, respecting Dr. Wilson's theory of the spots. I think Dr. Wilson is right when he says that the penumbra is made by the slopes of the collapse of luminous matter tending to fill the holes and close up the gaps in the solar atmosphere. The objections made to him that the penumbra is too sharply terminated at the limit from the brilliant part, I think, does not more subsist, after my observations made in the way suggested by Mr. Dawes. I have always seen the structure of the penumbra radiate towards the centre of the nuclei, and, what is very singular, that each radius, or rather streamlet, which composes the penumbra, is equally brilliant as the rest of the luminous body. The penumbra seems to me made by a mixture of streamlets of luminous matter with the dark intervals of the black ground which constitutes the nuclei. So we have an explanation of an almost invariable fact which has been a puzzle to every theory, namely, that the outline which separates the nucleus from the penumbra is similar to the outline which separates the penumbra from the luminous part. As I intend to develope this subject, after some more observations will be made; so, for the present, I will conclude by simply stating, that I do not mean to enter into the theory of the *production* of the spots, but only into that part which belongs to the *description* of them, and so I dare say Dr. Wilson's theory is the best. Sir J. Herschel's views respecting a circulation analogous to our trade-winds being the cause of the solar spots, seems to me to acquire new interest from the fact that the solar equator is hotter than the polar regions.

" The comet of the 6th of March has not been seen more since the 11th of April; and on the 5th I saw a star of the 11th magnitude through it. I would send you some observations of the new planet, but want of space prevents me. I will only add that I am very glad that Mr. Lassell also observed the different colour at the two sides of the black ring of *Saturn*, which I had seen several times last year (see our *Memoirs* for 1851, p. 36).* If the great refractor is ready next year, we shall be able to make some better observations, and to tread the path made by Mr. Lassell.

" *Rome, April* 28, 1853."

* This volume has not yet been received at the apartments of the Society.—R. G.

Letter from the Rev. W. R. Dawes to the Editor.

"Some suggestions have been made, and experiments tried, with the hope of imitating some of the phenomena of a total solar eclipse, and thus obtaining a sight of the red projections from the edge of the sun. It is, however, evident, that though the image of the sun itself may be completely and exactly concealed by some contrivance in the eye-piece, or permitted to pass through an aperture into a dark chamber, while the equatoreal movement is regulated precisely to apparent solar time, yet there will remain a great and, I fear, insurmountable obstacle, in the *luminosity of the earth's atmosphere* enlightened by the sun's rays. This difficulty appears to have been overlooked. If a telescope furnished with an eye-piece having a very small field, or half the field covered, be directed to the sky close to the border of the sun, the sun itself being just excluded, the glare is far too powerful to be endured by the eye without the interposition of a pretty deep tint of darkening glass. Such, at least, has been the case under the most favourable circumstances of clear and deep blue sky, during which I have been able to try the experiment.

"In the autumn of 1851, after my return from observing the total solar eclipse in Sweden, it occurred to me that the best chance of obtaining a sight of these phenomena might be afforded by taking advantage of that state of sky, lovely indeed, but too rarely seen in this country, where detached and sharply-defined clouds are slowly moving over the deep blue surface of the heavens. On two or three such occasions, the clock-movement of my equatoreal having been carefully regulated, I watched the passing of well-defined clouds over the sun, which certainly seemed to produce a rather darker tint of sky in the immediate vicinity of the sun's edge than any artificial contrivance in the eye-piece. Yet the advantage gained was not such as to give the slightest hope of success.

"If it is at all possible to render these extremely delicate phenomena visible by artificial means, it will probably be accomplished by erecting a suitable instrument on the top of one of the highest accessible mountains in a fine climate, like that of Oroomiah in Persia, as described by Mr. Stoddard, where the smaller density of the atmosphere and its great dryness exceedingly diminish its illumination by the sun's rays. A telescope of very moderate size might answer the purpose, if equatoreally mounted and driven accurately by clock-work.

"*Wateringbury, May* 6, 1853."

We are requested to state, that the observations on suspected variable light in the letter of the Rev. T. W. Webb, at page 186 of the last number, refer to the star *β Corvi*, not *Coronæ*, as there erroneously stated.

On the Orbit of the First Comet of 1850.

By R. C. Carrington, Esq.

The readers of the *Astronomische Nachrichten* will recollect that in No. 803 Mr. Sonntag, assistant at the Altona Observatory, published an orbit of Dr. Petersen's third Comet, which he had very elaborately deduced from a careful discussion of all the European and American Observations, about 400 in number. Mr. Sonntag's conclusion was, that the orbit was sensibly parabolic, the numerical deviation resulting from his equations of condition being many times less than its probable error. It was hardly to be expected, after this, that further observation and discussion would materially modify his result, yet such I have found to be the case, and the result is instructive; for though in ordinary cases it would excite no surprise that an excentricity should be elicited by an increase of the arc of observation, Mr. Sonntag's result appeared too exact and too well based to countenance the expectation.

The comet's excentricity was not, however, its only feature of interest. At the bottom of page 155 of vol. x. of the *Monthly Notices* will be seen the paragraph, "Mr. Gauss is even of opinion that it is not *proved* that a comet must, after having passed the perihelion, move in the same orbit as before." I cannot say that I apprehend the exact idea contained in this sentence, although I can read it in more ways than one; yet I think my calculation will afford a satisfactory answer to it. The orbit of this comet after perihelion fits perfectly to that before. This, although not a general proof, is a particular instance.

In residual phenomena, attributable to physical causes extraneous to gravitation, this comet offers "no case for a jury," the final uncompensated differences of the normals being of a magnitude attributable to observation.

My calculation is, strictly speaking, a combination of Mr. Sonntag's. I adopted at once his eight normal positions, not thinking a recomputation of the European observations would repay the time it would take: to those I was enabled to add two other normals, drawn from the very excellent observations made by Mr. Maclear himself at the Cape of Good Hope, Mr. Sheepshanks having kindly allowed me to take a private copy. For the whole period of visibility the perturbations of the six innermost planets were calculated and applied according to the new co-ordinate method of Professor Encke. In this part of the work I derived assistance, which I am happy here to acknowledge, from the annotated translation published by the Astronomer Royal in the *Nautical Almanac* for 1856. In this paper I pass at once to my result, as I have sent a fuller account to Dr. Petersen for insertion in the *Nachrichten*.

The elements represent the instantaneous orbit of May 12, 1850 (at Greenwich, noon):

T	.....	1850, July 23·565169	Berlin Mean Time.
π—N	.....	180° 31′ 36″·74	Mean Eq^{x}. 1850·0.
N	.....	92 53 27·84	
I	.....	68 11 23·56	
e	.....	0·9988519	
q	.....	1·0814488	Motion direct.

leaving the following uncompensated normal differences :—

		$\Delta\alpha \cdot \cos\delta$	$\Delta\delta$	
		″	″	
1.	May 10·0	+5·88	−1·88	
2.	21·0	−3·05	−0·86	
3.	30·5	+0·01	−0·93	
4.	June 7·5	−0·57	+0·67	
5.	21·0	−0·19	+1·67	
6.	July 2·5	−0·07	−0·70	
7.	12·0	+0·47	−0·54	
8.	21·0	−1·06	+1·01	
9.	Sept. 15·0	+1·28	−0·48	The Cape Normals.
10.	Oct. 5·0	−0·36	−0·65	

The period corresponding to the excentricity given above being somewhat short of 29,000 years.

Letter from M. Yvon Villarceau to the Astronomer Royal.

"I have the honour to transmit to you along with this letter a copy of a Memoir which I have just presented to the Academy of Sciences, upon the double star η *Coronæ*. I shall be gratified if you should find it worthy of being presented to the Astronomical Society.*

"Allow me to avail myself of this circumstance to give you some explanations upon a subject of controversy, which appears to have excited the attention of the Astronomical Society.

"In a memoir upon the comet of D'Arrest, I expressed the opinion that the observations of that body, made with the heliometer of Königsberg on the occasion of its last apparition, might be more precise than those which have been obtained elsewhere during the same period. M. Otto Struve appears to conclude from this remark, that, in general, I prefer heliometer observations of comets to those made with a refractor as powerful as that of Cambridge, and he declares that he does not concur with me in adopting this preference. The circumstance which induces him to reject the heliometer in its application to observations of comets consists in

* An abstract of M. Villarceau's Memoir will be found at p. 229.

the diminution of the brightness of the images, arising from the duplication of the object-glass.

"I suspect that my excellent friend, M. Otto Struve, has not rightly understood the meaning of my proposition, or, probably, the fault is attributable to myself. In a letter referring to subjects of a rather varied kind, and in which I communicated to him the results to which a discussion of the observations of D'Arrest's comet had conducted me, I did not insist sufficiently upon the point which has become the subject of controversy.

"With the view of fixing the meaning of my proposition, I divide into two classes the modes of procedure employed in observations of comets.

"First, the telescope is fixed in a position such that the comet and the star of comparison ought to present themselves successively in the field of view by the effect of the diurnal motion. With respect to the right ascension, the observation consists in estimating the instants of the passage of the comet and the star across fixed wires, insomuch that the difference of right ascension is based upon the appreciation of instantaneous phenomena.

"Secondly, the telescope is rendered movable in position by means of a clock movement, the star is fixed in the field of view, and the comet ought to be visible in the field at the same time. The observation, according to this method, is made in the same way as when the companion of a double star is observed: or, again, the comet may be considered to be connected by way of triangulation with several small stars visible at the same time in the field. This method, which has been already employed with success, especially by M. Otto Struve, differs from the former inasmuch as the measure of time no longer enters into the determination of the difference of right ascension. The bisection of the image of the comet may be made with the greatest facility by means of an apparatus fitted with wires: the observer may operate at his ease: the instantaneous phenomenon which offers difficulties of observation in the other method does not present itself here: and the time merely serves to fix the date of observation.

"I have discussed, in my Memoir, the different circumstances, according to which the sensibility of the organ of vision plays an important part in the determination of the right ascensions of very faint comets observed by the first method: there are cases in which the right ascensions ought, in general, to fall short of their true values. The optical power of the instrument cannot diminish this inconvenience, except when the comet has ceased to have a certain degree of brightness; but at the moment when it is about to disappear in large instruments, the inconveniences to which I am referring, ought to manifest themselves as well as in instruments of merely moderate power. In the latter, they will manifest themselves several weeks or months earlier, and the comet soon ceases to be observable, while large instruments allow of the observations being continued for a longer time. The errors of right ascension of very feeble comets which have their source in the manner in

which the retina is affected during the *passage* of a comet behind a wire, ought, according to my opinion, to disappear entirely if we employ the second method, in which the comet is bisected by a wire, and *remains* in that state during an interval of time, which is enormously longer than that of a passage.

"My opinion, then, is, that better results will be obtained by making use of the second method of observation; that which is, in fact, practised by M. Otto Struve himself. Now, it happens that, in the case of the comet of D'Arrest, the only instrument which has been applied in this way is the heliometer of Königsberg. It is not, however, the heliometer which I extol in so far as regards the comet, but rather the method above referred to. With respect to the heliometer in particular, it may be inferred that if the images are sufficiently bright to be observable without much difficulty, the results may be better than those which would be obtained by employing the first method even with refractors of sufficient power. Now, in the case of the comet of D'Arrest, the heliometer observations will be found to be as concordant as the others; the only difference being, that the observer was compelled to abandon his observations eight days earlier than Mr. Challis at Cambridge.

"If, now, we were to apply the second method, making use of a refracting telescope of the same power as the Northumberland refractor, the observations would unquestionably be more precise than those obtainable by means of refractors of the same dimensions, but fitted with divided object-glasses, since the intensities of the images are in the ratio of 2 to 1. Thus, let it be well understood, it is not the heliometer which I prefer, but the second method of observing comets.

"I regret much that I am unable to lay before you the grounds of the reasoning employed in my Memoir; possibly you might find that I have somewhat exaggerated the inconveniencies of the first method of observation. There is much to be said upon this subject, and, unfortunately, the arguments do not admit of being reduced to figures, which would tend very much to simplify the difficulties.

"Excuse, I pray you, this long digression, which I should not have thought of placing before your eyes, as too uninstructive, if I had not been led to it by the reading of the *Monthly Notices*.

"*Paris, April* 28, 1853."

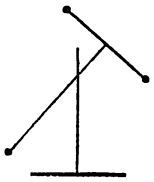
On the Mechanical Imitation of Precession. By the Rev. Professor Powell, F.R.S., &c.

The actual mechanical exhibition of the composition of rotatory motion giving the physical explanation of the Precession of the Equinoxes has been attempted by more than one contrivance.

In the *Astronomical Notices*, vol. i. p. 43, appears a description of a machine of this kind by Mr. Atkinson, but so brief that it is difficult to collect what the principle of it was. It is also under-

stood that another instrument was constructed by the Astronomer Royal; but of this it does not appear that any description has been published.

In the present instance, before the author was aware of either of these contrivances, he had constructed one which he believes exhibits the essential mechanical principle with greater simplicity than any previously devised.



It consists merely of a bar (loaded at its ends) capable of rotating freely about one end of an axis, which itself can turn about a point in its length, near the end carrying the bar, upon an horizontal axis capable of moving in azimuth round a vertical pillar. At the lower end of the first axis is a weight, which *rather more* than counterpoises the upper part; and when there is no rotation in the bar, simply brings the first axis down into a vertical position.

If this axis be held horizontally or obliquely, and a rotatory motion be given to the bar about it, on letting the axis go, the weight will no longer bring it down, nor alter its inclination at all, but will cause the whole to turn in azimuth, in a direction opposite to that of rotation.

Note on the Small Star near α Canis Minoris.

By Isaac Fletcher, Esq.

"In the Bedford Catalogue Captain Smyth* notes that the star *α Canis Minoris* is accompanied by a small companion of the eighth magnitude. The following is the result of the Captain's measures:—

1833·81 Pos. 85°·0; Dist. 145″.

"In 1848, Mr. Bond, of the Cambridge U.S. Observatory, announced that the small star was '*missing*.' In 1850 I saw and measured the position of the companion with ease, and estimated it as of the ninth magnitude. My measures gave this result:—

1850·17 Position 84° 19′.

"During the spring of this year I have looked most carefully for this small star with my 6-foot achromatic; but I have never obtained a trace of its existence.

"This star is undoubtedly a variable one of the most interesting description, and as such I beg to recommend it to those observers whose attention is directed to this class of phenomena.

"*Tarn Bank, Cockermouth*, 1853, *May* 5."

* Those Fellows of the Society who are not already cognizant of the fact, will be gratified to learn, that since Mr. Fletcher's note was written, the distinguished officer, to whose well-known work on astronomy allusion is here made, has been promoted to the rank of rear-admiral.

Discovery of a New Planet. By M. Luther.

(*Extract of a Letter from Professor Argelander to the Rev. R. Sheepshanks.**)

"I have great pleasure in announcing to you the discovery of a new planet on the 5th of May, by M. Luther, at Bilk. Unfavourable weather prevented a complete confirmation of the discovery before the 14th of the same month: hence the late announcement. M. Luther has only sent the following positions:

	Bilk M.T.	R.A.	Decl.
	h m	o ′	o ′
May 5	13	207 40	−10 15
14	11 52	206 3	9 57

To which may be added the following observations by M. Schönfeld:

	Bonn M.T.	R.A.	Decl.
	h m s	o ′ ″	o ′ ″
May 15	12 0 41·6	205 53 41	−9 55 46·5

"The planet is only equal to a star of the 10·11th magnitude: consequently, it cannot be observed longer in our telescopes. The Northumberland refractor, however, might show it long enough to obtain elements of sufficient accuracy to prevent its being lost. Would you have the kindness to request Professor Challis to undertake the charge of the friendly stranger?

"*Bonn, May* 16, 1853."

M. Luther states, in a communication to the *Astronomische Nachrichten*, announcing his discovery of the planet, that he hopes to be enabled to follow its track by the aid of the excellent charts of Messrs. Bishop and Hind.

Micrometrical Measurements of the Double Stars, ε Boötis, γ Virginis, 44 Boötis, ζ Boötis, and ζ Herculis, made at the Observatory, Whitehaven. By J. F. Miller, Esq. F.R.S.

ε Boötis.

Magnitudes, 3, 7, 5. Colours, yellow and sea-green.

Ref. No.	Position.	Weight.	Obs.	Distance.	Weight.	Obs.†	Epoch.
	o ′			″			
98	321 58	3	8	2·862	2	9	1852·279
124	321 1	4	8	2·839	3	8	1853·230
137	320 39	2	9	2·736	1	3	·304
Means	321 15	9	25	2·829	6	20	1852·937

"124. With this power the small star is obviously sea-green; though, with 250, it was pronounced to be bright blue. There is no certain evidence of change in this star, either in position or in distance.

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* In consequence of Mr. Sheepshanks having been recently absent from this country, on the occasion of a short visit to the Continent, he did not receive M. Argelander's letter till a day or two ago, when the present number of the *Monthly Notices* had been already made up, and was just about being printed off. It only remained, therefore, to extract the more important parts, and substitute them for other communications, the publication of which might be conveniently delayed till the issue of the next number. This circumstance accounts for a part of the communication being inserted here, and another part at page 238.

† "The distances throughout represent double measures, consequently each set really consists of sixteen observations."

γ Virginis.

Magnitudes, 3·2, 3·5. Both stars white.

Ref. No.	Position.	Weight.	Obs.	Distance.	Weight.	Obs.	Epoch.
	° ′			″			
123	175 7	4	8	3·114	4	8	1853·227
125	75 11	4	8	3·067	4	8	·241
126	175 15	4	8	3·167	4	8	·241
29	174 44	4	8	3·041	4	8	·243
130	174 58	4	9	3·274	1	8	·298
136	174 33	1	7	...	...	...	·304
139	174 20	2	8	...	...	...	·312
Means	174 54	23	56	3·108	17	40	1853·266
Means	175 17	11	48	3·122	7	24	1852·262
Means	175 54	7	15	3·040	3	8	1851·475

The upper star is still the brightest.

44 *Boötis.*

Magnitudes, 5, 5½. Both stars white.

Ref. No.	Position.	Weight.	Obs.	Distance.	Weight.	Obs.	Epoch.
	° ′			″			
127	240 25	4	8	4·291	4	8	1853·241
128	240 25	4	8	4·349	3	8	·241
134	239 20	3	8	4·361	2	9	·304
135	239 36	2	7	4·367	2	8	·304
140	240 52	4	8	4·370	4	8	·315
Means	240 34	17	39	4·349	15	41	1853·281

"The collected measures show that the stars have been gradually opening since the distance was first determined by Struve, in 1819.

	Position.	Distance.	Epoch.
	°	″	
Herschel I.	60·10	...	1781·62
Struve	228·0	1·50	1819·43
Do.	233·65	2·555	1829·20
Dawes	235·65	3·280	1833·39
Do. (Bishop's Cat.)	235·60	3·841	1842·39
Hind (Do.).....	237·99	4·261	1847·09
Miller	240·23	4·349	1853·28

ζ Boötis.

Magnitudes, 4·5, 4·5. Both stars white.

Ref. No.	Position.	Weight.	Obs.	Distance.	Weight.	Obs.	Epoch.
	° ′			″			
133	127 38	4	8	1·2 est.	...	...	1853·298
138	127 32	2	8	1·1 est.	...	...	·312
141	126 33	4	8	1·202	4	8	·315
143	127 25	4	8	1·181	4	8	·317
Means	127 14	14	32	1·191	8	16	1853·310

"A comparison of prior measures with the above appears to cate the fixity of this pair.

	Position. °	Distance. ″	Epoch.
Herschel I.	131·98	1·00	1796·60
Struve	129·28	1·19	1830·47
Dawes	128·28	1·32	1832·47
Do. (Bishop's Cat.)	127·03	...	1841·57
Do. (Do.)	127·03	1·039	1843·32
Miller	127·23	1·191	1853·31

"I cannot say that I have ever seen this star fairly divided before present spring.

ζ *Herculis.*

Magnitudes, 3, 7. Colours, A yellow; B purple.

No.	Position. ° ′	Weight.	Obs.	Distance. ″	Weight.	Obs.	Epoch.
	77 28	1	8	1·5 est.	1	...	1853·298
	78 49	4	8	1·454	3	8	·315
	78 38	4	8	1·534	4	8	·317
	79 40	2	8	1·422	1	8	·324
	78 8	2	9	...	...	...	·378
	78 25	3	8	...	...	...	·380
18	78 38	16	49	1·402	9	24	1853·335
18	84 4	4	16	...	...	...	1852·769
18	89 9	6	16	...	...	...	1851·650

"This elegant binary star, with the short period of thirty-five s, moves with such rapidity in its orbit, that the angular motion he acolyte is quite appreciable, year by year.

"The epochal results indicate an increase in the distance of years; and I believe the interval between the components is not far short of 1½ seconds.

	Position. °	Distance. ″	Epoch.
Herschel I.	69·30	...	1782·55
Struve	23·40	0·910	1826·63
Do.	196·99	1·094	1835·45
Do.	168·65	1·030	1838·44
Dawes (Bishop's Cat.)	150·67	1·230	1840·66
Do. (Do.)	129·89	1·298	1843·58
Struve	119·06	1·248	1845·27
Dawes	112·23	...	1846·83
Miller	89·15	...	1851·650
Do.	84·06	...	1852·769
Do.	78·63	1·402	1853·335

"The companion of ζ *Herculis* has been pretty steadily visible in my telescope during the last three years.

"Between the 24th of March and the end of April we were favoured with an uninterrupted continuance of the finest astronomical weather I almost ever remember, and it was during this period that most of the above results were obtained. The eye-piece employed was a new lens of admirable workmanship,—power 500, made by Mr. Simms, with which systems of the 3d and 4th magnitude (under illumination) presented perfect discs, with scarcely a trace of rings. Indeed, nothing could exceed the beauty and sharpness of the stellar definition with this eye-piece; and I prefer it to any lower power, when the atmosphere is at all tolerably steady.

"During the magnificent weather which has prevailed since the commencement of May, the astral definition has been so deplorable that I have only obtained three sets of positions in as many weeks, with the apparent advantage of an almost continuously bright and unclouded sky. Since the beginning of the month the air has been in an exceedingly dry state, the dew-point ranging from 12° to 22°·5 below the temperature of the air.* To the very unequal distribution of moisture over the upper and lower strata of the atmosphere the twirling, moulding, and blotted appearance of the stars is, no doubt, to be attributed.

"*Observatory, Whitehaven, May* 24, 1853."

On the probable Errors of the Eye and the Ear in Transit Observations. By the Rev. T. R. Robinson, D.D., President of the Royal Irish Academy.

With respect to the usual mode of recording transit observations, and the invention recently introduced at American observatories for accomplishing the same object by means of electricity, the author remarks, that *the two systems not merely employ different senses to co-operate with sight, but that the sight itself is used in two different ways.* In the "Electric Register" the eye catches the moment when the star crosses a wire, and the touch is to synchronise with it in giving a signal. In the common modes the ear counts the clock's beats; the eye notes the distances of the star from the wire at the beats which precede and follow its passage, and thus the mind estimates the fraction of the second from the relative position of three points; all existing in memory alone, when that estimation is made.

"The error, which may be caused by uncertainty in catching the precise part of the beat which shall be taken for the origin of the second, is the same for every star (at least, if we suppose the

* "To day (24th), at 3 p. m., the difference between the temperature of the air and of evaporation was 16°·3; and the complement of the dew-point, 24°·5 ! !"

clock equally audible in every position of the telescope, and the observer able to observe in all with equal convenience, for personal discomfort will interfere with the attention), that arising from uncertainty in estimating the star's place will vary inversely as the cosine of its declination.

"But there is yet another, arising from the actual displacement of the star's image, by irregular changes in the refractive density of the atmosphere: the effect will, as the preceding, be inversely as the cosine of declination, but also directly as the magnitude of these changes. This depends, in the first place, on the heterogeneity of the air as to heat and moisture; and, in the second, on the quantity of disturbed medium through which the line of sight passes. The former scarcely admits of expression in terms of our present meteorological data, and we must be content to assume for it an average value. In respect to the other, as the disturbance takes place chiefly within a small distance of the earth's surface, it will easily be seen that its amount is as the secant of zenith distance. If, then, we denote by u the probable error of the ear, by y that of the eye, and by z the atmospheric tremor at the zenith, we have, by the theory of these errors, for a star whose declination is δ,

$$\epsilon^2 = u^2 + \frac{y^2}{\cos^2\delta} + \frac{z^2}{\cos^2\delta \cdot \cos^2(l-\delta)}.$$

Hence, if we select three stars, properly differing in zenith distance, we can determine the three errors, u, y, z. By a process of this kind, executed in the year 1830, the author found, from observations of his own made at Armagh:

$$u = \pm 0^s{\cdot}0445; \qquad y = +0^s{\cdot}0619; \qquad z = \pm 0^s{\cdot}0381.$$

In the author's case, therefore, the ear could estimate the twenty-third part of a second, and its precision was to that of the eye as 7 : 5.

A second examination was instituted several years afterwards, certain alterations having been made in consequence of the first one.

"As the value of y was not very much less than the whole ϵ that I had obtained with the former eye-piece, notwithstanding its low power, a new one was obtained from the late Mr. Dollond, of 240, and very good. Nine lines were inserted by Mr. Grubb, finer and more uniform than the previous seven; the clock, whose arc of repose had been four times that of escape, had its weight changed from 4 lb. to 1·75; and a multitude of trees and shrubs were removed from the south of the transit-room, as their evaporation was a manifest cause of unsteadiness. All these, except the alteration of the clock, tended to lessen the error; that improved the rate, but it made the beat less audible in high winds, and therefore would increase u. In this instance, stars all of the first magnitude were selected. The results were,

$$u = \pm 0^s{\cdot}0732; \qquad y = \pm 0^s{\cdot}0554; \qquad z = 0^s{\cdot}0049.$$

The error of hearing is considerably increased, that of sight a little lessened; but the tremor is only a seventh part of its original amount."

"It is evident that the same equations apply to the new mode of observation, if u represent the error of touch; and it is to be hoped that a similar discussion of its probable errors may be soon instituted, for there are a number of inquiries which must be answered before its superiority will be fully appreciated. Independent of the possibility, that the means employed to close and break the voltaic circuit may disturb the clock's pendulum, and that the promptness of the register may vary with the intensity of the battery, it is certain that in the obedience of the finger to the will there must be a personal equation; and it is possible that this, unlike that of the ordinary transit observer, may be variable. For any regular succession it will probably be insensible, as I infer from some observations given by Mr. Mallet, in the Report of his valuable experiments on the propagation of earthquake waves, p. 306. Starting a chronograph at a given beat of a clock, and stopping it at another, he gives the differences from the mean; whence I compute ϵ, for himself $= \pm 0^s{\cdot}0449$, and for his son $\pm 0^s{\cdot}0592$. Each of these involves two errors of ear and two of finger, so that the measure of one of each is $0^s{\cdot}0318$ and $0^s{\cdot}0419$. These come so near to my u, that any error of touch is scarcely possible: the case is, however, a special one, and may be compared to the counting seconds without a clock, which a practised observer will do with surprising accuracy. Phenomena not regularly recurring could scarcely be taken so accurately.

"The error of sight will, for the reason already given, be less in the case of stars; as to the sun and moon, it is more doubtful. In the transit they have larger probable errors than the stars. For the sun I obtained, in 1830, the first limb $\pm 0^s{\cdot}116$, the second $\pm 0^s{\cdot}087$; the moon gave $\pm 0^s{\cdot}149$; while stars observed at the same hour, and near the same parallel, had but $\pm 0^s{\cdot}097$. This greater uncertainty arises from the strong contrast between the bright and dark surfaces whose boundary we take; and a similar one may be expected in attempting to note the precise instant of its passage.

"As to tremor, it will act here precisely as in transit observations; and it may, moreover, perhaps nullify one of the proposed advantages, that of making many observations in a few seconds. The undulations of the air are twofold,—those of short period, which cause the flutter that produces z, and those of much longer duration, which, without blurring the star, displace it, and sometimes cause the pole star, after crossing the wire, to go back, and after ten or even fifteen seconds make a second transit. In the present mode, such waves affect only a single wire, and may be neutralised by others; but in the other they would vitiate the whole set."

Note VI. on Double Stars. By M. Yvon Villarceau.

η Coronæ Borealis.

"The double stars whose elements may be regarded as approximately ascertained are four in number, viz. *ξ Ursæ Majoris*, *p Ophiuchi*, *ζ Herculis*, and *η Coronæ*. This number will be further reduced to three, if the double solution which I have announced in the *Comptes Rendus*, tome xxviii. p. 411,* should hold good till the present time. The object of this Note is to fix the one of the two solutions of the orbit of *η Coronæ*, which ought definitively to be adopted."

"It will be recollected that the ambiguity which I encountered arose from the circumstance that the two components of the system have so close a physical resemblance to each other, that it is impossible to distinguish the one from the other except by their relative positions. This distinction, which is possible when the positions compared differ slightly from each other, or are separated by a short interval of time, ceases to be so when they refer to distant epochs which do not embrace any intermediate observation. This difficulty presents itself in the case of two observations of Sir William Herschel made in 1781 and 1802, and distant from each other twenty-one years. An equal interval of time separates the latter from the epoch of 1823, whence commences a series of observations, extending without interruption down to the present day. The question arises, Ought the observations of Sir William Herschel to be employed in the same way as they have been originally presented, or ought they to be *reversed*, that is to say, modified to the extent of 180°? The condition of representing them by the same system of elements as the modern observations can alone decide this alternative."

M. Villarceau remarks, that by preserving the position of 1802 in his former researches, he was obliged to reverse that of 1781, and was hence conducted to an orbit characterised by a revolution of about sixty-six years, which no one had hitherto suspected. On the other hand, reversing the observation of 1802, and preserving that of 1781, he obtained an orbit of about forty-three years. This is the orbit which had been already announced by astronomers, and which is to be found in the greater number of the works which treat of the subject of double stars. Although a minute discussion of the physical circumstances of the two observations of Sir William Herschel suggested probabilities in favour of the orbit of sixty-six years, still, as a comparison of the two orbits did not assign any serious reasons for preferring either orbit, it was deemed expedient to refer to another epoch the question to be decided between the

* See also *Additions à la Connaissance des Temps pour* 1852.

two solutions. The year 1853 was fixed upon for this purpose; but it was announced by M. Villarceau that the two orbits might possibly be separated before that time, if the optical power of the great refractor of Pulkowa admitted of the observations being continued, notwithstanding the close approach of the component stars.

"The observations," says the author, "have been, in fact, continued till this time, and the interest excited by the subject has induced Messrs. Lassell and Hartnup to make observations in England, which they have had the kindness to communicate to me. Mr. Dawes has also been kind enough to transmit to me a series of unpublished observations, extending for η *Coronæ* down to 1849 inclusive. This circumstance enables me to present already the results which I have arrived at in so far as regards the choice between the two orbits. I have also availed myself of the new observations to correct the elements of the definitive orbit, the occasion for executing which process I pointed out at the conclusion of my first communication."

"At the first glance it was easy to see that the series of observations made since the year 1847 could not agree with the orbit of 43 years. I accordingly applied myself to the correction of the elements of the orbit of 66 years. There now occurred a difficulty which the insufficiency of the data had prevented me from encountering in my former researches, and which had already presented itself in my investigation of the orbit of ζ *Herculis*, but in a form less pronounced; I refer to the employment of distances in determining the orbits of stars so close to each other. Determining the elements of ζ *Herculis* by the aid of angles of position alone, I succeeded in representing these angles satisfactorily enough, but the distances exhibited systematic errors. Employing simultaneously the angles of position and the distances, the latter were better represented, but systematic errors appeared in the angles of position."

"In my new researches upon η *Coronæ* an attempt to correct the elements, by employing simultaneously the angles of position and the distances, has conducted me to results which leave strongly-pronounced and inadmissible errors in the comparison of the angles of position. Taking into consideration the great precision of the measures of the angles of position, I then resolved to employ solely the latter, and to reserve the measures of distance for the determination of the semi-axis major alone. On this point I may remark, that five measures of distance executed by M. W. Struve from 1826 to 1835 exceed $0''.7$; while seventeen other measures, executed subsequently by the same astronomer and by M. Otto Struve and M. Mädler, are comprised between $0''.6$ and $0''.4$. When any other elements beside the semi-axis major are to be deduced from the measures of distance, the results depend especially *upon the variations of distance*. Now it is evident that the variations of distance comprised between $0''.6$ and $0''.4$ are quite comparable with the errors that may affect measures of such delicacy. With respect to the other five distances comprised between $0''.7$ and

1″·15, the employment of their variations also presents difficulties to which I shall presently refer. I have consequently been induced by these considerations to employ exclusively the angles of position in the correction of all the elements except the semi-axis major."

M. Villarceau has determined the semi-axis major of the orbit from the measures of distance executed by M. W. Struve, M. Otto Struve, and M. Mädler. With respect to the distances of M. W. Struve, which are less than 0″·8, and which, as in all other similar cases, were obtained by mere estimation, M. Villarceau has applied to them a small correction, the necessity of which was suggested to M. Struve by experiments on the distances of artificial stars, and originally announced by him in his *Mensuræ Micrometricæ*. M. Villarceau has also found it necessary to apply a similar correction to the distances comprised between 0″·4 and 0″·6 which were observed by M. Otto Struve, at Pulkowa; and also to two observations of distance executed by M. Mädler, at Dorpat.

The observations of angles of position which have been employed in correcting the elements of the orbit of η *Coronæ*, are in number 37, comprising all those with which the author was acquainted. The equations of condition have been treated by the method of least squares. The following are the new elements deduced :—

Passage of the Perihelion	1779·338; 1846·647	
Mean Annual Motion	5°·3484	
Angle of Excentricity	23° 51′·0	
Longitude of the Ascending Node	9° 52′·3	Counted from the Meridian of 1850.
Longitude of the Perihelion	194° 51′·9	
Inclination	±59° 18′·6	
Semi-axis Major	1″·2015	

Whence

	yrs.
Period of Revolution	67·309
Excentricity	0·40433

These elements differ only in a slight degree from those given in the *Connaissance des Temps* for 1852. The greatest angular difference, that relative to the position of the node, is 5° 31′; the time of revolution has been augmented by about a year; and the date of the passage of the perihelion by about three months.

The following numbers exhibit the results of a comparison of the corrected elements with the totality of the observations. In the column of observers, the names of the two Herschels are denoted by W. H. and J. H.; those of the Struves by W. S. and O. S.; the affix J. H. and S. indicates the joint observation of Sir John Herschel and Sir James South. The names of Messrs. Dawes, Hartnup, Lassell, and Mädler, are denoted by the letters D, H, L, and M.*

* The Table of M. Villarceau is more complete than that given in the text; but we are compelled from want of space to omit several of the columns.—Ed.

Date.	Observer.	Observed Angle of Position − Cal. Angle. Dihedral.	In Arc.	Cor. Distance − Cal. Dist.
		° ′	″	″
1781·7	W. H.	−0 12	−0·002	—
1802·7	id.	+1 5	+0·028	—
1823·3	J.H.&S.	−1 32	−0·038	—
1826·8	W. S.	+0 24	+0·008	−0·055
1829·6	id.	+0 19	+0·006	−0·061
1830·3	J. H.	−1 11	−0·030	−0·151
1831·4	D.	+1 10	+0·018	—
1831·4	J. H.	+2 12	+0·034	—
1831·6	W. S.	−0 38	−0·010	+0·019
1832·5	J. H.	+1 8	+0·016	—
1232·6	D.	+0 45	+0·011	—
1832·8	W. S.	−0 14	−0·003	+0·027
1833·3	J. H.	+1 53	+0·025	—
1833·4	D.	+2 36	+0·034	—
1835·4	W. S.	−2 23	−0·026	+0·157
1836·5	id.	+0 39	+0·006	+0·089
1837·5	W.&O.S.	−4 7	−0·038	−0·025
1838·4	id.	−5 17	−0·048	−0·028
1839·8	O. S.	−3 45	−0·035	+0·144
1840·5	O. S.	−1 46	−0·017	+0·
1841·4	M.	+0 25	+0·004	+0·
1841·5	O. S.	+0 31	+0·005	+0·
1842·2	M.	+0 1	0·000	−0·
1843·3	O. S.	−2 44	−0·031	+0
1845·6	W.&O.S.	−2 2	−0·025	−0
1846·6	O. S.	+1 45	+0·022	−0
1846·9	D.	+2 41	+0·033	−
1847·6	O. S.	+2 8	+0·026	−0
1848·0	D.	+1 41	+0·020	−0
1848·7	O. S.	−0 22	−0·004	−0
1849·4	D.	+3 45	+0·041	+0
1849·7	O. S.	−1 54	−0·020	+0
1850·5	id.	−4 3	−0·040	−0
1851·4	H.	+1 7	+0·010	−
1851·4	L.	+3 9	+0·029	−
1851·6	O. S.	−5 0	−0·045	+0
1852·6	id.	+4 41	+0·040	+0

"We now proceed," says the author, "to explain the mode in which we have determined the semi-axis major. The other elements obtained by means of the angles of position alone enable us to calculate the different values of the ratio of the distance to this element. Let ϱ' denote this ratio, A the semi-axis major, A ϱ the distance observed and corrected conformably to the indications of M. Struve, we have then for each observation of distance an equation of the form $\varrho = \varrho' A$.

"Treating by the method of least squares the totality of the equations founded on the observations of M. W. Struve, and submitting to a similar process the observations of M. Otto Struve and M. Mädler, we obtain the three following equations:—

By 8 Observations of M. W. Struve.....	4″·3217 = 3″·5770 A;	whence	A = 1″·2082
12 — of M. O. Struve.....	3·7067 = 3·1063 A;	—	A = 1·1933
2 — of M. Mädler.....	0·5908 = 0·4903 A;	—	A = 1·205
By the totality of 22 Observations.....	8·6192 = 7·1736 A;	—	A = 1·2015

"The greatest deviation of these three determinations from the final result is only 0″·008. So close an agreement tends to justify the application which we have made to the observations of MM. Otto Struve and Mädler, of the corrections which were legitimately applicable only to those of the elder Struve."

"The two measures of distance obtained by Mr. Dawes were omitted, because, in consequence of the feeble optical power of his

instruments compared with those of Dorpat and Pulkowa, the measures of very small distances did not seem to be readily comparable with those made in Russia. Still the errors — 0″·025 and +0″·073 deduced from a value of the semi-axis major obtained without employing the observations of Mr. Dawes, show that the observations of that distinguished astronomer are not so far from being comparable with those of MM. Struve as one would at first have supposed. The semi-axis major which the two measures of Mr. Dawes give is 1″·241,—a value which differs only by 0″·040 from the definitive value. Moreover, it is to be borne in mind that this result being deduced from quantities which are almost a half less than the result obtained, the influence of errors of observation is almost doubled."

"It will be seen from the foregoing table that the greatest residual error of the angle of position reduced to arc does not exceed 0″·048. Their totality gives for the probable error of an angle of position expressed in the same way 0″·0188. Now M. Struve has established that the probable error of the mean of three observations of angle of position reduced to arc varies from 0″·018 to 0″·028 for the distances comprised between 0″·70 and 1″·48. The result above given accords very well with the experimental determinations of M. Struve.

"There appear, in the foregoing table, traces of a systematic error between the epochs 1837 and 1840. It is to be remarked, however, that this interval comprises *a passage of the apparent perihelion*; besides, there are no other observations than those of MM. Struve. Again, from 1846 to 1848 there appear several errors affected with the same sign, but which, however, are very small. Finally, the rather sensible errors of the two last observations may be attributed to the close approach of the two stars; for the companion is advancing towards the apparent perihelion which it ought to have passed in 1853·377, the distance being reduced at that epoch to 0″·484."

"It now remains to show how the orbit of forty-three years satisfies the observations. We shall confine ourselves to a comparison of the two last observations of M. Otto Struve with the elements (second solution) published in the *Connaissance des Temps* for 1852. The result of this comparison is:—

Date.	Obs. Angl.—Cal. Angl.	Obs. Dist.—Cal. Dist.	Cor. Dist.—Cal. Dist.
	°	″	″
1851·56	— 104·71	— 0·035	+ 0·083
1852·62	— 91·30	— 0·213	— 0·093

"A more approximate value of the deviations of the two orbits may be obtained, in so far as the angles of position are concerned, by eliminating from the observations of M. Otto Struve the errors indicated by the comparison exhibited above. In this manner here results:—

	° ′	
1851·56;	— 99 7	
1852·62;	— 96 11	
or about	— 97 5	for the beginning of 1852.

"Now, a discordance of this magnitude cannot be made to disappear by a slight change in the elements. We are then authorised to reject the orbit of forty-three years of revolution, and to consider the orbit of sixty-seven years as being that which the companion of η *Coronæ* really describes.

"The revolution of 67·309 years does not appear to me to be susceptible of being sensibly modified by ulterior observations, seeing that the position corresponding to the ancient observation of Sir William Herschel has been attained and surpassed within the last few years. The period of revolution is henceforward affected only by an uncertainty, at the utmost equal to the time during which an angle of position would be described corresponding to the algebraic difference of the mean error of the modern observations and that of Sir William Herschel. It will be easily seen that this uncertainty cannot exceed a year. Now, the number 66·257 years already satisfies the totality of the observations anterior to 1848. It is plain, therefore, that the true time of revolution, if it exceed 67·309 years, cannot deviate from it more than a fraction of a year.

"I may add, in conclusion, that having applied to the observations of MM. Struve alone, the method presented in my third Memoir upon double stars, without subjecting them to a correction relative to the distances, I have obtained immediately an orbit, still somewhat indeterminate, it is true, but in which the time of revolution was found to be 69·3 years. Thus, without having recourse to the ancient observations of Sir William Herschel, the series of observations of MM. Struve suffices already to give an approximate idea of the orbit of η *Coronæ*."

Instructions and Suggestions for Mariners observing the total Solar Eclipse of Nov. 30, 1853.*

(Appended to a Letter from M. Jul. Schmidt, of the Observatory of Bonn, to Lieut. Maury, U.S.N., and communicated by Admiral W. H. Smyth.)

Every mariner navigating the Pacific Ocean in the neighbourhood of the line of central eclipse is requested (if possible to make observations) to direct his attention to the following points:—

I.—Observations with the Telescope during the total Eclipse.

1. Note the duration of total darkness in seconds of sidereal or mean time.

2. Note the time when, before the beginning of the eclipse, *the corona* (white ring round the dark moon) makes its appearance.

3. Note the time when, in the reappearance of the sun's light, the corona disappears.

4. Observe, if the corona is circular, if it has rays; and if so, are they perpendicular to the moon's limb?

* This eclipse will be total along a certain belt of the Pacific Ocean between the coast of Peru and the Sandwich Islands.

5. Note the diameter of the corona in parts of the moon's diameter.

6. Note the colour of corona.

7. Observe how many *red flames*, prominences, or protuberances, are visible.

8. On what points (counting eastward from the highest point) on the moon's periphery are the prominences situated?

9. Estimate the extent in length and breadth, if one or more prominences, either in minutes, of space or in parts of the moon's diameter.

10. Do the prominences on the east side of the moon decrease and disappear?

11. Do those on the west side increase, and disappear with the reappearance of the sun's light?

12. Does there appear at the beginning and end of total darkness a *red limb* (a series of small and combined red flames)?

13. Give a sketch as accurate as possible of the forms and relative situations of the prominency.

14. Note the colour or tint of the red flames.

15. Are the prominences red at the commencement of their visibility, or do they redden later?

16. What other appearances in the vicinity of the red flames in the corona?

17. Are there in the nearest vicinity of the sun's limb to the prominences *solar spots*?

18. Note the colour of the moon's disc.

II.—Observations without a Telescope during the whole Eclipse.

1. Note the changes in the colour of the sky during the increase and decrease of the eclipse, as well as the totality.

2. Note the variations of the temperature of the atmosphere.

3. Note the variation in the direction of the wind.

4. Observe the dew.

5. Observe the behaviour of sea-birds in the vicinity of the vessel.

6. Observe the colour of the sea, before, during, and after the total darkness.

7. How many stars, and of what magnitude, besides the most splendid, are visible during the totality?

8. Are the prominences visible to the naked eye?

9. What is the colour and form of the corona as seen with the naked eye?

It must be left to the direction of the navigator which of the above observations he, in accordance with the means in his power, will make himself, and which he will distribute among his subordinates.

Lastly, it is desirable for each observer to note—

a. The longitude and latitude in which the observations have been made.

b. Whether the telescope inverts or not, as well as its magnifying power.

J. F. JULIUS SCHMIDT,
Astronomer.

Royal Observatory at Bonn, Prussia,
Feb. 19, 1853.

Tables du Mouvement apparent du Soleil, deduites de la Comparaison de la Théorie avec les Observations faites depuis 1750 *jusqu'à nos Jours.* Par M. Le Verrier.*

M. Le Verrier having, about two years since, executed a complete theory of the perturbations by which the apparent motion of the sun is affected, was subsequently induced to institute a comparison between his theory and an extensive series of observations of the sun. These researches have resulted in his construction of new solar tables, forming the subject of his present communication to the Academy of Sciences. The most remarkable part of this communication is the announcement of an irregularity in the motion of the solar perigee, which it seems impossible to account for by the existing theory of the sun's motion.

M. Le Verrier proposed to establish his investigation of the correction of the elements of the solar orbit upon the observations of the sun made by Bradley at Greenwich, about the middle of the last century, and the modern Greenwich observations. The results thus deduced might be verified by comparing them with a series of observations made about the middle of the included interval.

He selected for the earlier materials of his investigation 719 observations of the sun made by Bradley between the years 1750 and 1762. The modern data consisted of 516 observations, made under the superintendence of the present Astronomer Royal between the years 1840 and 1850. These observations were reduced by the aid of the fundamental catalogue of stars recently constructed by M. Le Verrier.

Having determined the secular variations of the excentricity and the perigee by comparing their values as derived from the two sets of observations, and taking into account the included interval of time, extending to ninety years, M. Le Verrier obtained results which appeared to him incompatible with any admissible change in the masses of the disturbing planets.

With the view of throwing light upon the subject, he had recourse to intermediate observations. For this purpose he chose 631 observations of the sun, made at Greenwich, Paris, and Palermo, during the years 1801–2–3–4. A rigorous discussion of these data served only to increase his perplexity. The correction of the ex-

* *Comptes Rendus*, tome xxxvi. p. 349.

centricity did not indeed offer any difficulty, being equal to the mean of the corrections found for the years 1755 and 1845, as ought necessarily to be the case, if the correction varied according to the first power of the time. With respect to the correction of the perigee, however, the case was quite different. Instead of being equal to the mean of the values found for 1755 and 1845, it was very considerably larger than either. Thus it appeared that the secular variation of the solar perigee, as deduced from the observations, besides being incompatible with the admitted values of the masses of the disturbing planets, presented the additional difficulty of not being simply proportional to the time.

A further appeal to observation seemed to offer the only means of arriving at some definitive solution of this question. M. Le Verrier accordingly had recourse to 241 observations, made at Greenwich during the years 1783–85, and to 1037 observations made at Greenwich, Paris, and Königsberg, during the years 1814–18. The number of observations of the sun which formed the groundwork of his researches consequently now amounted to 3144. By a comprehensive and searching discussion of the totality of these data, he finally arrived at the conclusion that the variable correction of the solar perigee might be represented by the following formula:

$$-7''{\cdot}9 + 0''{\cdot}535\,t + 29''{\cdot}8 \sin(5^{\circ}{\cdot}4\,t + 207^{\circ}{\cdot}85)$$

in which t expresses the number of years, counting from the beginning of 1850. The following table exhibits a comparison of the results derived from this formula, with the corresponding results of observation:

Date.	Correction of the Solar Perigee deduced from the Observations.	Calculated Correction.	Calculation —Observation.
	″	″	″
1753·3	−39·4	−38·5	+0·9
1759·0	−25·3	−27·7	−2·4
1784·4	−56·4	−59·3	−2·9
1801·5	−62·3	−58·1	+4·2
1803·5	−46·5	−53·3	−6·8
1815·3	−20·9	−16·1	+4·8
1817·7	− 8·1	− 8·6	−0·5
1842·8	− 2·7	− 6·0	−3·3
1848·2	−24·4	−18·2	+6·2

From the close agreement which subsists between the observed and calculated corrections of the solar perigee, as exhibited in the foregoing table, M. Le Verrier considers himself fully authorised in announcing that, independently of the movement, the cause of which is known to us, *the solar perigee is subject to an oscillation whose amplitude is 60″ and whose period is 66⅔ years.* He further concludes, that, besides this periodic oscillation, the solar perigee is subject to a secular variation of 53½″ which is not produced by the action of known masses. The excentricity of the solar orbit offered

indications of a similar variation, but of a magnitude much less pronounced.

Without professing to announce a positive opinion on the subject, M. Le Verrier seems disposed to think that the combined action of the minor planets may not impossibly be the cause of these irregularities.

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*Elements of Themis.** By MM. Forster and Krüger.

(*Communicated by Professor Argelander to the Rev. R. Sheepshanks.*)

The following elements of the planet *Themis* were calculated by MM. Krüger and Forster, from two observations made at Naples on the 6th and 13th of April, and two observations made at Bonn on the 27th of April and the 10th of May.

Epoch 1853, May 10·43140, Berlin M.T.

M =	341	52	26·8
π =	213	32	44·2
Ω =	32	26	35·3
i =	0	53	46·8
φ =	14	21	14·2
Log a =	0·561038		
Log μ =	2·708450		

A comparison of these elements with the observations gives the following results:—

		Theor. — Obs. $\Delta\lambda$	Theor. — Obs. Δl
April	6	−0″·4	+31″·6
	13	−0″·2	− 0″·9
	27	+1″·0	− 0″·8
May	10	+0″·1	−18″·8

Accompanying these elements was an ephemeris of the planet, calculated by M. Krüger, and extending from May 20 to June 17. Any Fellow of the Society may obtain a copy of this ephemeris, by applying at the Apartments of the Society.

* This name has been selected for the planet by Sig. Secchi, at the invitation of the discoverer, M. De Gasparis. It is the same name which M. De Gasparis originally intended to confer on the planet *Massalia*.

ERRATA.

Vol. XIII. Page 87, line 1 from top, *for* 1852 *read* 1853.
— — 156, — 22 — *for* Stoddart *read* Stoddard.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIII. *June* 10, 1853. No. 8.

G. B. AIRY, Esq., President, in the Chair.

P. Kernan, Esq., 21 Clifford Street, Bond Street;
Dr. Roxburgh, 38 Gloucester Place, Portman Square;
E. H. R. Coleman, Esq., 9 Wood Street, Cheapside; and
Thomas Finch, Esq., 5 Promenade Villas, Cheltenham,

were balloted for and duly elected Fellows of the Society.

Sig. Secchi, Director of the Observatory of the Collegio Romano at Rome, and
M. Yvon Villarceau, of Paris,

were duly elected Associates of the Society.

Discovery of a New Comet. By M. Klinkerfues.

On the 10th of June, M. Klinkerfues, at the Observatory of Göttingen, discovered a small comet with a tail 3′ or 4′ long. The following are the observed positions:—

1853.	M.T. Gött. h m s	R.A. h m s	Decl. ° ′ ″	Stars of Comparison.
June 10	13 0 53·6	9 31 53·77	+43 28 17·1	Lalande, 18953
11	11 25 3·0	9 32 29·97	+43 23 57·9	— 19022

For elements and an ephemeris of this comet, see *Ast. Nach.* No. 864, p. 390.

Observations of a Comet made on board H. M. S. Centaur, by Lieut. Goodenough, R.N.

(*Extract of a Letter to the Astronomer Royal.*)

"As it appears to me that the following observations of a comet seen here may not be uninteresting, I beg to enclose them.

"The comet was first seen at $6^h\ 45^m$ p.m. April 30, a little south-west of *Orion*, bearing west-south-west. Its nucleus was very distinct from the coma, and shone as a star between the 3d and 4th magnitude. The coma extended (by estimation) about 2′ round the nucleus; and the tail, pointing nearly direct to the zenith, had a length of about 4°, its width increasing but little.

" An observation was taken as follows, at a little after seven p.m.,—

Green. M.T.	True Dist. for Sirius.	True Dist. for Castor.
h m s	° ′ ″	° ′ ″
11 9 37	34 43 50	58 22 33

" Both observations are corrected for refraction, they give a R.A. $4^h\ 22^m\ 20^s$, and Decl. 6° 45′ 2″ South.

" The evening of May 1 was cloudy, and only one observation could be obtained, at

Green. M.T.	True Dist. from Rigel.
h m s	° ′ ″
11 32 2	5 12 30

the comet being in a direct line with λ *Leporis* and *Rigel*.

" The mail leaves this evening, so I send these two; in the event of obtaining any further observations to-night, I will send them. I believe you prefer to receive the observations themselves. I have therefore sent them corrected only for refraction.

" *H.M.S. Centaur, Buenos Ayres, Outer Anchorage, May* 2, 1853."

" Our position is four miles north-east of the fort of Buenos Ayres."

Remarks on Saturn as seen with the Madras Equatoreal (by Lerebours and Secretan; focus, 89 in., aperture, 6.2 in.) By W. S. Jacob, Esq., Director of the H. E. I. Co.'s Observatory, Madras.

The faint inner ring and the division in the outer ring were both seen with power 365, on 24th August, 1852, soon after the arrival of the new object-glass (the original one having been defective), and they have been viewed on various subsequent occasions with powers from 174 to 365; the fine outer division can, under favourable definition, be traced through more than one-half the circumference; its breadth cannot be estimated at more than one-third or one-fourth of the large division, say 0″·1. The faint ring as projected on the planet has a filmy appearance, and the planet is seen through it as through a film of smoke; it has an umber brown tint; off the planet it has a slaty hue. The large division is also decidedly not black, but little, if at all, darker than the faint ring as seen on the planet, and of about the same tint; its inner edge is pretty sharp, but its outer edge seems rather shaded off. No belts were seen on *Saturn*, except the broad bright equatoreal one; the whole south hemisphere being shaded or mottled over not quite uniformly, but a little darker next the white belt, and also in slight cloudy patches here and there, especially towards the pole. The shadow of the planet on the ring is very black and sharp. An eclipse of *Tethys* is thought to have been seen on 5th January. The satellite had been tolerably conspicuous near the western ansa, but as *Saturn* neared the zenith, it was noticed as fainter, and on looking for it a few minutes later it could not be seen. The time of disappearance would be about $2^h\ 10^m$ Madras

real time, but with an uncertainty of several minutes. The llite was seen again at about 5^h, nearly opposite *Saturn's* north

The following are the results of four nights' measures, reduced *aturn's* mean distance (9·5430, according to Bouvard's tables), ers used 365, 365, 277, 277. Mean date of observation, 3·0123, Greenwich mean time.

Outer Diameter of Outer Ring	39·91
Diameter of fine Division	38·09
Inner Diameter of Outer Ring	35·40
Outer Diameter of Inner Ring	34·67
Inner Diameter of Inner Ring	26·32
Inner Diameter of Faint Ring	22·19
Equatoreal Diameter of Saturn	17·86
Polar Diameter of Saturn	16·51
Breadth of large Division	0·37
Minor Axis of Outer Ring	14·33

Saturn's equatoreal diameter was corrected for phase (0″·04.) above measures agree moderately well with Struve's, as given *st. Soc. Mem.* vol. iii. p. 299, but differ considerably from the es in the *Nautical Almanac*. My breadth of the interval veen the rings is 0″·04 less than Struve's, and is decidedly too ll (owing to the shading off of the outer edge noticed above), apparent breadth being *estimated* at fully 0″·5, or little, if at all, than the thickness of the micrometer wires which is 0″·6. I not been able to make out any other marks or divisions in the than those mentioned above.

On the Parallax of α Herculis. By W. S. Jacob, Esq.

(*Extract of a Letter to the Rev. R. Sheepshanks.*)

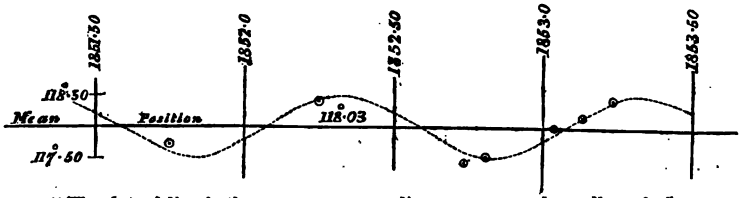
'I write in haste to save a mail, and give you early notice of observations of *α Herculis*, proving, as I think, the existence of rallax of about 0″·06. I send only the mean results, and a h tracing of the curve, as the details will appear in the forth- ing volume of our *Memoirs;* but it is desirable that the atten- of astronomers should be called to the star, and that my results ild if possible be confirmed by some one else. I am just ing Madras for the hills on account of my health, so that my rvations will be interrupted, but I hope to take the subject up in on my return.

"The following are my mean results. You see the course of curve is pretty well represented, not only by the observations r the extreme points, but also by the intermediate ones. The rvations of distance are, of course, not exact enough for de- ing so small a quantity, but their evidence is at least not con- ictory. The instrument used was the Lerebours' equatoreal in. focus, $6\frac{1}{4}$ in. aperture), power in the two first sets 200, being

the highest the maker had furnished with his micrometer. The remainder were taken with a Dollond's micrometer (kindly lent by General Fraser), and with powers of 277 and 365. By far the greater number of observations were by daylight, or I doubt if they would have given such consistent results.

Epoch.	Position.	No. of Obs.	Distance.	No. of Obs.
	°		″	
1851·753	117·73	20	4·57	30
1852·252	118·43	25	4·45	30
·736	117·44	25	4·38	24
·801	117·59	35	4·60	42
1853·039	118·03	21	4·54	18
·127	118·13	23	4·63	24
·268*	118·52	26	4·46	26

" Epochs for greatest effect of parallax on the position, ·327 and ·835, or 30th April and 1st November.



" The dotted line is the curve corresponding to an annual parallax of 0″·059.

" *Observatory, Madras, April 12th*, 1853."

On the Determination of the Latitude of the Liverpool Observatory by means of Observations made in the Prime Vertical. By John Hartnup, Esq.

A short time subsequent to the establishment of the Liverpool Observatory, W. H. Simms, Esq., the present Surveyor-General of Ceylon, in a letter which I received from him, incidentally mentioned that he had been getting some observations on the prime vertical for latitude, and that he had found so convenient a formula for correcting the errors in the position of the transit instrument that he could apply it to all stars, whether high or low, with almost the same results. At my request he very kindly sent me the formula, and I applied it in the reduction of a series of observations taken with a transit placed nearly in the prime vertical, for the purpose of determining the latitude of the Liverpool Observatory. This formula has not, I believe, been published; and, with the permission of the author, I, therefore, beg to present it to the Royal Astronomical Society, together with fifty-eight results of co-latitude, in the reduction of which it has been employed.

The following are the formulæ which I received from Mr. Simms:—

* Includes an additional observation taken this morning, 13th April.

φ, the co-latitude of the place of observation.
ϖ, the polar distance of a star observed on the east vertical.
ϖ', — — — — west vertical.
H, the hour-angle of the star observed east.
h, — — — — west.
μ, the inclination of the axis of the instrument, positive when the south end is the highest.
ν, the azimuthal deviation from the prime vertical, supposed to be small.

$$\left.\begin{aligned}\tan k &= \tan \varpi \,.\, \cos \mathrm{H}\\ \sin \psi &= \sin \varpi \,.\, \sin \mathrm{H}\end{aligned}\right\} \quad \left\{\begin{aligned}\tan k' &= \tan \varpi' \,.\, \cos h\\ \sin \psi' &= \sin \varpi' \,.\, \sin h\end{aligned}\right.$$

$$\nu = \frac{\cos \psi \,.\, \cos \psi' \,.\, \tan (k' - k)}{\sin (\psi + \psi')}$$

$$\varphi = k \pm \mu \pm \tan^{-1} (\nu \,.\, \tan \psi).$$

In the determination of the Valentia Arc of Longitude a very fine portable transit instrument of $3\frac{1}{2}$ feet focal length and $2\frac{1}{4}$ inches aperture, made by Troughton and Simms, was used at the Valentia station. The instrument is the property of the Rev. R. Sheepshanks, who very kindly allowed it, on its return from Ireland, to be left at the Liverpool Observatory for the purpose of determining the latitude by transits over the prime vertical.

A massive stone pier for the reception of the equatoreal was at that time erected; the walls of the equatoreal-room were built up, and a temporary covering of wood was thrown over the room. On this massive stone pier the portable transit stand was placed, and arrangements were made for me to walk around the instrument without stepping upon the pier. The transit instrument thus placed nearly in the prime vertical was within hearing of the sidereal clock, the error of which was determined by transits taken with the meridian transit instrument.

In order to give the method a fair trial I decided, previous to commencing the observations, that the following conditions should be complied with:—

1. That the time of taking each set of observations should not extend beyond an interval of a few hours; this was done with the view of guarding, as far as possible, against changes in the azimuthal position of the instrument.
2. That for each set, at least one star should be observed east, and one west, so that a good determination of the value of ν might be obtained.
3. That the instrument should be levelled as often as convenient during each set.
4. That every observation not made under favourable circumstances should be at once rejected, and not reduced.
5. That no observation made under favourable circumstances and reduced, should be rejected on the ground of discordance.

Agreeably to the above conditions, 58 transits over the prime vertical were observed and reduced, 22 over the west, and 36 over the east vertical. Sixteen different stars were observed, whose north polar distances range from 38° to 66°, and the extreme difference in colatitude between either of the 58 results and the mean result is only 2″·66, and there are 31 of the 58 results which do not differ from the mean to the amount of one second of arc. The places of the stars were all taken from the Greenwich 12-year Catalogue.

The following example, which is added as a specimen of the calculation, will show the great convenience of the method. The three stars observed followed each other so closely that the whole time occupied in observing them did not extend over an interval of ten minutes, and the latitude deduced from these three observations differs only from o″·37 from the mean of the 58 results.

1845, *February* 6. *Illuminated End of Axis South.*

	12 *Can. Ven.*, W.	β *Draconis*, E.	γ *Draconis*, E.
	h m s	h m s	h m s
Observed Transits	16 19 53·37	16 25 24·00	16 28 41·60
Clock slow	12·69	12·70	12·70
Transits corrected	16 20 6·06	16 25 36·70	16 28 54·30
App. R.A. of Stars.....	12 48 48·04	17 26 55·23	17 52 59·64
Hour-Angle in Time ...	3 31 18·02	1 1 18·53	1 24 5·34
	$h = 52^\circ\ 49'\ 30''·3$	$H = 15^\circ\ 19'\ 37''·95$	$H = 21^\circ\ 1'\ 20''·1$
	$\pi' = 50\ 50\ 58''·7$	$\pi = 37\ 35\ 7''·0$	$\pi = 38\ 29\ 37''·0$

ν *determined by* 12 *Can. Ven.*, W., *and* γ *Draconis*, E.

$\tan \pi' = 0·0893017$	$\sin \pi = 9·9005058$	$k' = 36^\circ\ 34'\ 57''·56$
$\cos h = 9·7812169$	$\cos H = 9·9700869$	$k = 36\ 35\ 14·41$
$\tan k' = 9·8705186$	$\tan k = 9·8705927$	$k' - k = -16''·85$
$\sin \pi' = 9·889578$	$\sin \pi = 9·794089$	$\cos \psi = 9·9888895$
$\sin h = 9·901346$	$\sin H = 9·554768$	$\cos \psi' = 9·895560$
		$\tan k' - k = 5·912175$
$\sin \psi' = 9·790924$	$\sin \psi = 9·348857$	$5·796630$
		$\sin (\psi + \psi') = 9·890905$
$\psi' = 38^\circ\ 9'\ 49''$	$\psi = 12^\circ\ 54'\ 7''$	$\log \nu = 5·905725$

12 *Can. Ven.*

$k' = 36^\circ\ 34'\ 57''·56$	$\tan \psi' = 9·895364$
$\tan^{-1}(\nu \cdot \tan \psi) = +13·05$	$\log \nu = 5·905725$
$\mu = -·85$	$5·801089$
$\varphi = 36\ 35\ 9·76$	

γ Draconis.

k	$= 36^\circ\ 35'\ 14''{\cdot}41$	$\tan \psi$	$= 9{\cdot}359961$
$\tan^{-1}(\nu \cdot \tan \psi)$	$= -\ 3{\cdot}80$	$\log \nu$	$= 5{\cdot}905725$
μ	$= \quad -{\cdot}85$		$5{\cdot}265686$
φ	$= 36\ 35\ 9{\cdot}76$		

β Draconis.

$\tan \pi$	$= 9{\cdot}8863183$	$\sin \pi$	$= 9{\cdot}785288$	$\tan \psi$	$= 9{\cdot}213154$
$\cos \cdot H$	$= 9{\cdot}9842717$	$\sin H$	$= 9{\cdot}422149$	$\log \nu$	$= 5{\cdot}905725$
$\tan k$	$= 9{\cdot}8705900$	$\sin \psi$	$= 9{\cdot}207437$		$5{\cdot}118879$
k	$= 36^\circ\ 35'\ 13''{\cdot}80$	ψ	$= 9^\circ\ 16'\ 41''$		
$\tan^{-1}(\nu \cdot \tan \psi)$	$= \quad -2{\cdot}71$				
μ	$= \quad -{\cdot}85$				
φ	$= 36\ 35\ 10{\cdot}24$				

In the determination of ϕ in the above example, it has been assumed that the instrument was free from error of collimation. I had no means of determining the error of collimation except by observations of stars made with the illuminated end of the axis north and south alternately, and it was found to be desirable to observe sets in both positions of the instrument, over three, five, and sometimes seven wires, and consequently the collimation differed in these different sets. In the above example, the stars were observed over three wires, and the collimation for three wires, with the illuminated end of the axis south, was found to be $+2''{\cdot}73$, therefore $2''{\cdot}73$ must be added to the results in the above example.

When a star passes the prime vertical at 50° or 60° from the zenith, a small error either in the assumed place of the star or in the observed transit, will affect the latitude considerably; and as a general rule it may, perhaps, be desirable to use very low stars for the purpose of determining the azimuthal deviation only. The following table has been compiled from the partial results in order to show the resulting latitude from each star:—

	N.P.D.	Latitude.	No. of Obs.
	° ′	° ′ ″	
β Draconis	37 35	53 24 48·03	4
θ Ursæ Majoris	37 37	46·92	4
γ Draconis	38 29	48·15	4
η Ursæ Majoris	39 55	46·78	2
α Persei	40 42	46·49	10
ι Ursæ Majoris	41 22	46·83	1
Capella	44 10	47·72	6
β Aurigæ	45 5	48·65	1
α Cygni	45 16	47·59	4
12 Can. Ven.	50 51	47·54	4
α^2 Geminorum	57 47	48·17	3
ζ Cygni	60 24	48·34	5
β Tauri	61 32	48·88	4
β Geminorum	61 36	48·22	3
α Andromedæ	61 66	49·87	2
ι Leonis	65 31	48·68	1

The mean latitude from all the observations $=53^\circ\ 24'\ 47''{\cdot}72$. The latitude of the transit instrument of the Liverpool observatory, by the Ordnance Survey, $=53^\circ\ 24'\ 46''{\cdot}34$ by the first method, and $53^\circ\ 24'\ 46''{\cdot}45$ by the second method. The position from which the transits over the prime vertical were taken is six feet south of the meridian transit instrument.

*Latitude of the Liverpool Observatory, deduced from Transits over t Prime Vertical.**

Date.	Name of Star.	Illuminated End of Transit Axis.	Vertical.	Hour Angle observed.	Azimuthal Deviation Correction.	Level Correction.	Collimation Correction.	C L tt
1844.				° ′ ″	″		″	°
Dec. 20	ζ Cygni	S	W	65 2 40·35	−61·31	−0·64	+3·07	36 35
	Capella	...	E	40 10 52·20	+24·09	...	...	
	β Aurigæ	...	E	42 15 34·95	+25·92	...	...	
	β Geminor.	...	E	66 21 41·85	+65·14	...	...	
	α^2 Geminor.	...	E	62 7 31·35	+53·92	...	...	
	ι Leonis	...	E	70 15 48·60	+79·46	...	...	
	α Persei	...	W	30 18 18·75	−16·68	...	...	
30	β Tauri	N	E	66 15 15·45	−31·75	−0·22	−3·07	
	α Persei	...	E	30 18 20·85	− 8·34	−0·06	...	
	ζ Cygni	...	W	65 4 9·30	+30·96	+0·14	...	
	Capella	...	E	40 9 6·75	−12·87	+0·92	...	
	β Geminor.	...	E	66 20 6·30	−37·38	+1·87	...	
	α^2 Geminor.	...	E	62 5 51·45	−31·12	+2·07	...	
1845.								
Jan. 11	φ Persei	S	E	30 18 40·20	−10·56	+2·02	+3·55	
	Capella	...	E	40 9 15·60	−15·24	+2·96	...	
	β Geminor.	...	E	66 20 11·25	−41·24	+3·30	...	
	α Androm.	...	W	66 31 4·80	+41·57	+3·42	...	
	α Persei	...	W	30 20 3·60	+10·56	+3·80	...	
	ι Ursæ Maj.	...	E	32 30 51·90	−11·50	+4·22	...	
12	α Cygni	N	W	42 49 29·40	+26·23	−0·56	−3·55	
	β Tauri	...	E	66 14 43·35	−64·69	−0·70	...	
	α Persei	...	E	30 17 41·85	−16·63	−0·80	...	
	ζ Cygni	...	W	65 4 39·90	+61·22	+0·03	...	
	Capella	...	E	40 8 33·00	−24·94	+0·07	...	
	α^2 Geminor.	...	E	62 5 25·80	−55·83	+0·73	...	
	α Androm.	...	W	66 31 20·70	+68·04	+0·94	...	
	α Persei	...	W	30 19 54·00	+17·30	+1·78	...	
18	β Tauri	S	E	66 15 7·35	−49·28	+0·69	+3·55	
	α Persei	...	E	30 18 27·00	−12·67	...	...	
	ζ Cygni	...	W	65 4 37·65	+46·63	...	...	
	α Persei	...	W	30 19 54·00	+12·68	...	...	36 35

* The original of the annexed table contained a column exhibiting the uncorrected co-l which has been omitted for want of space.

Name of Star.	Illuminated End of Transit Axis.	Vertical.	Hour Angle observed.	Azimuthal Deviation Correction.	Level Correction.	Collimation Correction.	Co-Latitude.
			° ′ ″	″	″	″	° ′ ″
β Tauri	N	E	66 14 30·60	−79·11	−0·35	−3·55	36 35 11·42
α Persei	...	E	30 17 30·00	−20·34	...	...	13·66
ζ Cygni	...	W	65 4 56·40	+74·89	...	...	11·42
α Persei	...	W	30 20 0·75	+20·36	...	...	12·19
β Draconis	S	E	15 18 55·80	− 9·63	+3·52	+2·73	12·14
γ Draconis	...	E	21 0 44·10	−13·51	+1·78	...	10·32
α Cygni	...	E	42 38 46·65	−32·40	+2·38	...	11·41
β Draconis	...	W	15 21 19·80	+ 9·66	+1·78	...	10·81
12 Can.Ven.	N	W	52 50 5·25	+37·82	−0·43	−2·73	10·25
β Draconis	...	E	15 18 17·70	− 7·85	−0·44	...	11·98
γ Draconis	...	E	21 0 7·95	−11·01	−0·44	...	12·02
α Cygni	...	E	42 38 45·15	−26·42	+0·69	...	11·36
α Cygni	N	E	42 39 3·00	−16·23	−1·48	−2·73	12·56
γ Draconis	...	W	21 1 42·30	+ 6·77	−1·48	...	12·56
12 Can.Ven.	S	W	52 49 30·30	+13·05	−0·85	+2·73	12·49
β Draconis	...	E	15 19 37·95	− 2·71	...	...	12·97
γ Draconis	...	E	21 1 20·10	− 3·80	...	...	12·49
Capella	N	W	40 9 33·60	+ 4·37	+0·21	−4·09	13·85
ℓ Ursæ Maj.	...	E	15 35 12·00	− 1·41	...	..	12·64
12 Can.Ven.	...	E	52 48 34·95	− 6·83	...	...	13·86
ℓ Ursæ Maj.	...	W	15 35 29·40	+ 1·45	...	...	13·18
η Ursæ Maj.	...	W	27 27 17·55	− 2·69	...	...	12·98
Capella	S	W	40 9 49·95	+ 1·11	+1·30	+4·09	13·25
ℓ Ursæ Maj.	...	E	15 36 23·10	− 0·37	...	...	13·32
12 Can.Ven.	...	E	52 48 57·75	− 1·74	...	...	13·25
ℓ Ursæ Maj.	...	W	15 36 29·85	+ 0·37	...	..	13·17
η Ursæ Maj.	...	E	27 27 59·85	− 0·69	..	..	36 35 13·45

Hence mean value of co-latitude = 36° 35′ 12″·28
and latitude ..... = 53 24 47·72

iverpool Observatory, 1853, *May* 31.

At the meeting of the Society, held on the 13th of May, a letter was read which Captain Manners had recently received from Mr. Wackerbarth of Upsala, containing information respecting the present state of astronomy in Sweden, and also some interesting particulars relative to the ancient observatory of Tycho Brahé in the Island of Huena.

Extract of a Letter from Sig. Secchi to Capt. R. H. Manners, R.N.

"I have lately received the interesting Memoir of M. Otto Struve on the rings of *Saturn*, and I have sought whether the ancient observations of Campani and the early Italian astronomers confirm what has been found by him, that the dark space between the ring and the planet formerly appeared larger than now, and I have found from a figure of Campani that this is really the fact. The figure, of which I send you the original, shows also a faint shadow as a vestige of the inner dark ring which has been lately discovered.

"*Rome, May* 24, 1853."

[The drawing of *Saturn* above referred to by Sig. Secchi, may be seen at the apartments of the Society. It is stated underneath that it was executed by Campani in the year 1664. The maximum breadths of the ring, and of the interval between the ring and the ball, measured roughly with a pair of compasses, appear to be *exactly equal*. The faint shading indicative of the obscure ring is seen *immediately contiguous* to the bright ring at the part where the latter crosses the body of the planet.]

Note on Professor Powell's Communication in the last Number of the Notices, p. 221.

Since the publication of the *Notice* referred to, the author has become acquainted with an apparatus on the same principle as that he has described, though differing in the details of its construction, which is more complex, invented by Bonenberger; a modification of which has been employed in Magnus's researches on some phenomena of rotatory motion in projectiles (see Taylor's *Foreign Scientific Memoirs*, N. S. part iii. p. 223). An apparatus somewhat of the same kind, he is also informed, is in the collection at King's College, London.

June 13, 1853.

Occultation of ο Tauri observed at Ashurst. By Robert Snow, Esq.

(N. Lat. 51° 15′ 58″. W. Long. 1^{m} 10^{s}·0.)

	h	m	s		
Immersion	6	23	30	Corrected sidereal time.	Bright limb
Emersion	7	33	15	— —	Dark limb

Immersion not good. Emersion very good.

Nov. 27, 1852.

On the Determination of the Longitude of the Observatory of Cambridge by means of Galvanic Signals. By G. B. Airy, Esq., Astronomer Royal.

The President resigned the Chair to Dr. Lee, and then gave a *vivâ-voce* account of the operations for determining the Longitude of the Observatory of Cambridge by Galvanic Signals.

The object for which the galvanic connexion of the Royal Observatory with the London Bridge Railway Station was originally proposed, was the determination of differences of longitude with other observatories, British and Continental. Till within a short time, however, the connexions have been made available, principally, for the transmission of daily signals at certain definite hours along the principal Railways of England: for the dropping of the Signal Ball mounted by the Electric Telegraph Company on the roof of their Office in the Strand: and for the maintenance of the movement of several sympathetic Clocks, one of which is at the London Bridge Station. As soon, however, as the proper turn-plates were mounted at the Royal Observatory, and other details of mechanical arrangement were completed, experimental comparisons were made (comparing the transit-clock at the Royal Observatory with a chronometer at London Bridge, with one at Dover, &c.) in order to learn by experience the most advantageous method of conducting the operation for a real determination of difference of longitude.

The wire from London to Greenwich used in these operations is susceptible of such connexions, that its current may be made to pass through the coils of a telegraph needle which is fixed on the case of the transit-clock, and thus to the earth. This is the connexion which is used when signals are to be received from a distant station. The wire also admits of such connexion that the galvanic circuit, interrupted at one place in the Royal Observatory, passes from the earth through a galvanic battery, then through the coils of the telegraph-needle, then (with an interruption as is mentioned above) to London; and the interruption may be destroyed, or the circuit completed, either (on one arrangement of turn-plates) by an apparatus similar to that of the common speaking-telegraph; or (on another arrangement of turn-plates) by a contact-piece on the eye-end of the transit-circle, which is intended ultimately for use in the American method of recording transits. In either of these cases, if the London or distant end of the wire communicates through the coils of a telegraph-needle to the earth, the completion of the circuit at Greenwich will produce a movement both in the needle at Greenwich and in that of the distant station.

The arrangement of Greenwich turn-plates which, in fact, was used in the observations to be described, was that in which the circuit is completed at the eye-end of the transit-circle. And some of the signals given did actually correspond to the transits of stars over wires of the transit-circle, as observed by the signal-giver, whose eye was applied to the eye-piece, while with his finger he made the contact with the contact-piece.

In the arrangement which has been described, it will be perceived that nothing depends on the accuracy of the signal-giver. It is his business to complete the circuit, and thus to give signals at certain approximate times; but by this operation needle-signals, similar in all essential respects, are given both at Greenwich and at the distant station; and if observers are stationed to observe the motions of the needles, the comparison of the clocks will depend

on the accuracy of these signal-observers only, who are under the very favourable circumstance of observing signals of similar character at both ends of the line. In order to give to these observations all practicable accuracy, it is desirable that the signal-giver should be at a distance from the signal-observer; and even that, if possible, he should be in another room.

The arrangements made at the distant stations were formed on the same principles, and, in fact, approached very nearly to those which have been described as applying to Greenwich. In one respect, however, they were necessarily less perfect: in no other place is the galvanic-needle carried by the transit-clock; and it was necessary, therefore, at other places to use another clock or chronometer, which was compared by means of transportable chronometers with the transit-clock.

The preceding description shows that at each station it is necessary to have a signal-giver (A) and a signal-observer (B). But it was also found advantageous to employ a third person (C), on account of the following arrangement. The plan adopted as most convenient was, that the signal-giver should fix, in his mind, upon any arbitrary number of signals for each "batch!" and that he should give notice of this number of signals to the signal-observers at both ends by giving the same number of warning-signals at intervals of about 2^s; (3^s is better for a long line.) It was the duty of (C) to watch and count these warning-signals, and then to say "There are [so many] signals coming;" he then remained silent while the signals, at intervals of 10^s to 15^s, were observed by the signal-observer; and when the just number was completed he called out, "The batch is finished." This left the signal-observer (B) free to examine the seconds on the dial, to write down the minutes, and generally to prepare himself for the next batch.

Supposing the signals given in equal numbers from the two ends of the line, the signal-giver (A) at each end was employed during only half the time; but (B) and (C) at each end were employed through the whole time.

The order of operations then, as between Greenwich and Cambridge, was as follows:—

At 11^h p.m. Greenwich Mean Solar Time, Greenwich commenced by giving five signals at intervals of about 2^s each. The turn-plates were changed, and Cambridge responded by five similar signals. These were merely to say "All is right." Then Greenwich gave batches of signals as above described, in numbers of from three to nine signals in a batch (some of them being transits of stars), to $11^h\ 15^m$. Then Cambridge gave similar batches to $11^h\ 30^m$. Then Greenwich gave signals to $11^h\ 45^m$, and Cambridge to $12^h\cdot0$. This closed the night's signals. From 135 to 150 efficient signals were given; and as the observation of a signal is perhaps rather less accurate than the observation of a transit-wire, the probable error of the mean of these will be fairly comparable with that of the determination of clock-error in an evening's transits.

The evenings selected for the determination of the longitude of Cambridge were those of 1853, May 17 and 18. Mr. Walker,

Engineer and Superintendent of Telegraphs of the South-Eastern Railway, undertook to make the circuit perfect in every respect from Greenwich to Lothbury; and Mr. Edwin Clark, Engineer of the Electric Telegraph Company, provided carefully for the junctions at Lothbury and the Eastern Counties' station, and for the instrumental and personal aid required at the Cambridge station. To these gentlemen the thanks of Astronomers are due; not simply for their assistance in the present operation, but for the energetic assistance which they have given in the important work of placing the Royal Observatory in galvanic connexion with the rest of England, and making its determinations of time generally useful. Professor Challis arranged the more strictly astronomical part of the operation, in the following manner. On May 17, Mr. Dunkin observed transits and galvanic signals at Greenwich, and Mr. Todd observed at Cambridge. On the morning of the 18th the observers were interchanged, and Mr. Todd observed transits and signals at Greenwich, while Mr. Dunkin observed at Cambridge. The errors of the transit-clocks were determined by two methods: method (A) in which the *Nautical Almanac* stars were employed, the same corrections being applied to the right ascensions of the *Nautical Almanac* for both observatories, but no particular care being taken for the identity of the stars at the two stations; and method (B), in which the clock was corrected by stars in a list suggested by Professor Challis, the same stars being observed at both stations, but no attention being given to the accuracy of the assumed Right Ascensions.

For comparison of the Cambridge transit-clock with the chronometers used at the Railway Station, Professor Challis employed three chronometers. It was found necessary to reject the comparisons of one of these.

It is unnecessary to say that every care was given to the determination of the instrumental errors of the transit-instruments at both stations. At both stations, transits were observed both before and after the galvanic signals, on both evenings. The number of battery-cells used at each station was 72. The air was dry, and the insulation was good.

The results of the operation are as follows:—

East Longitude of Cambridge.

Method (A).

	s
May 17, by 145 signals	22·953
May 18, by 134 signals	22·978
Mean	22·966

Method (B).

	s
May 17, by 145 signals	22·903
May 18, by 134 signals	22·988
Mean	22·946
Mean of the whole	22·956

This is probably one of the most accurate determinations of difference of longitude hitherto made.

The result arrived at in 1829, by transmission of chronometers, was $23^{s}.54$.

The President then stated that on May 25 signals were passed (in exactly the same manner) to and from Edinburgh, for the determination of the longitude of Edinburgh Observatory, and, as regarded the galvanic part of the operation, with perfect success. There was, however, something suspicious in the indications of one of the chronometers employed at Edinburgh, which threw doubt on the clock-comparisons, and rendered the apparent determination of difference of longitude doubtful. The following result however was obtained, which is free from the influence of uncertainty in the chronometer-rate; that when a signal is given at Greenwich, by means of a Greenwich battery, the time noted for the signal at Edinburgh is later than that noted at Greenwich by $\frac{1}{17}$th of a second of time (and *vice versâ* if the signal is given at Edinburgh by means of an Edinburgh battery). This difference probably arises from two causes: first, the time actually occupied by the transmission of the galvanic pulse, which, according to the American determinations, would explain less than half of the difference; secondly, the circumstance that the galvanic current when it reaches the distant needle is somewhat less vigorous than when it passes the nearer needle, and the languid movement of the distant needle catches the eye more slowly, and is recorded as occurring at a later time.

In reply to a remark by Mr. R. C. Carrington, on the influence of the length of the transit telescope upon the determination of clock error, the President stated that the length of the Greenwich transit telescope is less than 12 feet, and that at Cambridge 10 feet, and that no sensible error would result from that cause.

On Certain Results of Laplace's Formulæ, expressing the Relation between the Inclinations, Excentricities, and other Elements of the Planetary Orbits. By Dr. Lardner.

In this paper the author shows that the well-known formulæ of Laplace,

$$\Sigma (m \sqrt{a}\, e^{2}) = c \qquad \text{(I)}$$

$$\Sigma (m \sqrt{a} \tan^{2} i) = c' \qquad \text{(II)}$$

would not of themselves suffice to assure the stability of the planetary system in the sense in which the term is usually applied. By a very simple process he demonstrates that for anything contained in these theorems to the contrary, the excentricities of all the planets which revolve within the orbit of *Jupiter* may attain any magnitude whatever, that their several orbits may actually become

hyperbolic, and that although the formula (II) certainly imposes major limits upon the inclinations, these limits are in some cases so considerable as to be inconsistent with the stability of the planetary system as usually understood.

We shall obtain the numerical value of the constant c by calculating severally the products $m \cdot \sqrt{a} \cdot e^2$ for each of the planets; but in the case of the group of small planets between *Mars* and *Jupiter*, their masses being undetermined, cannot with any arithmetical precision enter into this calculation: it is evident, however, that, from their extreme minuteness, they will produce a very inconsiderable effect upon the value of c. It may be sufficient for the present purpose to suppose the group to be replaced by a single planet, having a mass equal to that of the earth, an excentricity equal to that of *Mars*, and a mean distance = 2·6. If this be assumed, and the several products $m \sqrt{a}\, e^2$ be calculated from the tables of the planetary elements, we shall find the following numerical results:—

	$m \sqrt{a}\, e^2$.
Mercury	0·00000001298
Venus	10
Earth	79
Mars	400
Planetoids	3947
Jupiter	504525
Saturn	277266
Uranus	38197
Neptune	2219
$\Sigma (m \sqrt{a}\, e^2)$	= 0·00000827941

It appears, therefore, that

$$c = 0{\cdot}00000827941.$$

If it be desired then to ascertain the greatest possible excentricity of each planet which is compatible with this constant value of c, it will only be necessary to suppose that $e = 0$ for all the other planets. That will give

$$e'^2 = \frac{c}{m' \sqrt{a'}},$$

where e' is the excentricity, m' the mass, and a' the mean distance of the planet, whose limiting excentricity is sought.

If, then, we substitute in this formula the numerical value of c determined above and the values of e, m, and a for each of the planets severally, we shall find the major limits of the values of e for the planets, including the imaginary planets taken to represent the group of planetoids.

	Major limit of e.
Mercury	5·192
Venus	1·976
Earth	1·714
Mars	4·240
Planetoids	1·350
Jupiter	0·0617
Saturn	0·0970
Uranus	0·2170
Neptune	0·1684

It appears, therefore, that for all the planets included within the orbit of *Jupiter*, the major limits of e are greater than 1, and, therefore, the excentricities are susceptible of all possible values between 0 and 1 and of all values between 1 and the several limiting values given above.

It follows from all this that, consistently with the conditions involved in the formula (I), the orbits of these planets may be ellipses of any degree of excentricity from the circle to the most flattened oval, and that they may be even hyperbolas, for which e would have any values between 1 and the several numbers given in the preceding table.

In the case of the four superior planets, *Jupiter*, *Saturn*, *Uranus*, and *Neptune*, the formula does impose such narrow limits upon their excentricities as must effectually guarantee their stability and the uniformity of their climatological conditions.

The formula

$$\Sigma (m \sqrt{a} \tan^2 i) = c'$$

is subject to precisely the same observations.

By substituting, as before, the values of m, a, and i, for each of the planets taken from the tables of the elements, we shall obtain the values of the products $m \sqrt{a} \tan^2 i$ for the planets severally, and by their addition we shall obtain the numerical value of their constant sum. Thus we obtain:—

$$\Sigma (m \sqrt{a} \tan^2 i) \ldots\ldots = 0{\cdot}000003141\overline{70}$$

To find the limiting value of each inclination it will be sufficient, as before, to suppose all the orbits but the one whose limiting inclination is sought to coincide with the plane of the ecliptic. If, then, m', a', and i, express the mass, mean distance, and inclination of the planet whose limiting inclination is sought, we shall have

$$\tan^2 i = \frac{c'}{m' \sqrt{a'}}.$$

Now, by substituting in these successively for m' and $\sqrt{a'}$ their values for each of the planets, we shall obtain the limiting values of the inclination, which will be as follows:—

	Limit of i.
	° ′
Mercury	72 38
Venus	50 36
Earth	46 34
Mars	69 3
Planetoids	39 45
Jupiter	2 11
Saturn	3 25
Uranus	7 37
Neptune	5 56

It appears, therefore, that in this case, as in that of the excentricities, the conservative influence of the formula only bears upon the outer group of planets, and that the inclinations of the inner group are liable, so far as they are restricted by this formula, to a range of variation altogether incompatible with all idea of conservation.

[Laplace, in the Memoir in which he first announced the theorems above referred to (*Mém. Acad. des Sciences*, 1784), remarked that the results of his researches on the secular variations of the planetary movements (to which these theorems formed direct corollaries) assured the permanent oscillation of the excentricities and inclinations of the orbits within comparatively narrow limits; and this statement was repeated by him in much more explicit terms in the *Mécanique Céleste* (tome i. liv. ii., tome v. liv. xv.) It is important to bear in mind, however, that, although these theorems do not imply quite so much as has been ascribed to them, this circumstance does not in the slightest degree impugn the researches of geometers on the stability of the planetary system. Lagrange, by a comprehensive investigation of the subject, first determined the actual limits within which the planetary orbits would oscillate from the mutual action of the disturbing bodies (*Mem. Acad. Berlin*, 1782), and the researches of that great geometer have been perfected and extended in recent times by M. Le Verrier. Laplace, in the Memoir already referred to, showed that the expressions for the secular variations of the excentricities and inclinations of the planetary orbits would consist of a series of terms involving the sines and cosines of angles proportional to the time, but would not contain any term involving the time either in an algebraic or an exponential form; and as he supposed the coefficients of the various terms to be small from the nature of the problem, he hence concluded that both the excentricities and the inclinations would perpetually oscillate between narrow limits. These researches were subsequently published in an improved form in the *Mécanique Céleste*, but on neither occasion were the actual limits of the excentricity or the inclination in any individual case determined; nor is any allusion made in the latter work to the remarkable researches of Lagrange on this subject, either in the first volume, wherein the subject of

the secular variations is formally investigated, or in the fifth volume, containing historical notices of the labours of Newton and his successors on the theory of gravitation (with the exception of perhaps a very obscure allusion to the subject at page 319 of tome v.) It may be stated that the inadequacy of the analysis of Laplace, to establish the stability of the planetary orbits individually, without having recourse to numerical calculation, had been already pointed out by Le Verrier and others (*Comptes Rendus*, 1840, 1^ier^ Semestre, p. 526).—R. G.]

Results of Micrometrical Measures of Double Stars made at Tarn Bank, Cumberland, from 1850·2 *to* 1853·4. By Isaac Fletcher, Esq., F.R.A.S.

The author commences his paper, by referring the reader to the tenth volume of the *Monthly Notices*, for a description of the instrumental means employed by him in obtaining the micrometrical measures here presented to the Society.

The telescope employed is a 6-foot achromatic refractor of 4·14 inches aperture, mounted equatoreally in the English fashion on a polar axis of mahogany, and driven by clock-work. The mounting, in fact, appears to be almost precisely similar to those of the instruments of Mr. Bishop, Lord Wrottesley, and the one which formerly belonged to Admiral Smyth, but which now adorns Dr. Lee's Observatory at Hartwell. The object-glass seems to be of first-rate excellence, as is abundantly shown by the measurement of such stars as *ζ Herculis*, *ϑ Cygni*, &c., and by the fact that, under favourable circumstances, it bears a power of 600. The measures have all been executed with a parallel wire micrometer, and a deep red illumination of the field of view. The hour-circle is *loose* on the polar axis,—an arrangement which appears to be convenient and advantageous, and Mr. Fletcher strongly recommends its general adoption. The entire instrument is the workmanship of Mr. Cooke of York. The catalogue of observations incorporates about 5200 *individual* measures, which are represented by about 280 *sets* of measures, most of the sets containing eight measures of position and *sixteen* measures of distance. In consequence of the greater liability to error in observing distances, Mr. Fletcher seems to have taken great pains to insure good results by a large accumulation of measures of this element.

Stars of an ascertained binary character seem chiefly to have occupied Mr. Fletcher's attention; and, accordingly, the catalogue contains numerous observations of *γ Virginis*, *ξ Ursæ*, 70 *Ophiuchi*, *Castor*, *ζ Herculis*, and other stars of interest. The catalogue is followed by a synopsis of *final* mean results.

Mr. Fletcher gives notes on some of the more remarkable objects to which his measures have reference, comparing his results with those obtained by other astronomers.

We here insert some of these notes:—

36 *Andromedæ.* Σ 73.

This object has changed its position nearly 30° since it was first measured by Struve, and possibly the distance may have very slightly increased since that time, as it was occasionally *separated* by my refractor.

			Position		Distance
			°		″
Struve.....	1832·14	Position	307·80	Distance	0·847
Dawes, B*	1841·81	—	320·75	—	1·076
Fletcher	1851·93	—	336·38	—	1·116

γ *Andromedæ.* Σ 205.

I was induced to pay considerable attention to this star, because Captain Smyth's measures seemed to indicate a pretty regular increase in distance. My result, however, is smaller than any of the Captain's. During the measures my instrument showed a slight elongation of the companion in the direction 116° ∓, and in November 1851 I saw it *completely divided* in Mr. Pattinson's magnificent refractor of $7\frac{1}{4}$ inches aperture.

μ *Canis Majoris.* Σ 997.

I have no doubt a change has taken place both in angle and distance, since Struve discovered this very beautiful but somewhat difficult object.

			Position		Distance
			°		″
Struve	1831·20	Position	343·53	Distance	3·223
Smyth	1834·15	—	342·9	—	3·5
Mädler.....	1844·17	—	340·80	—	3·133
Fletcher	1852·60	—	338·07	—	2·947

The angle seems retrograding and the distance diminishing

Castor. Σ 1110.

My observations compared with other recent ones of this object, show that the distance is still slightly augmenting, and the angular motion diminishing, thus showing the general accuracy of the orbits which have recently been calculated by Mr. Hind and M. Mädler. The angles deduced from my observations are not, however, very closely represented by M. Mädler's ephemeris, as will be seen from the following comparison,—

	Observed.		Calculated.	
	°	″	°	″
1851·04	Pos. 248·67	Dist. 5·074	Pos. −2·45	Dist. −0·098
1852·04	247·97	5·074	−2·36	−0·077

That this difference is not attributable to much extent to error

* The "B" attached to some of the results by Mr. Dawes, signifies that they were obtained in Mr. Bishop's observatory.

in any observed angles, is shown by the correspondence of my first result with that of Mr. Dawes for the same year, which is,—

1851·21	Position	248°·12	Distance 5″·068

γ Virginis. Σ 1670.

In 1851 I calculated an orbit for this star, which is published in vol. xi. of the *Notices* of the Society. Since then I have obtained additional measures for two years, and using all other results that have come to my knowledge combined with my own, I have proceeded on Sir John Herschel's graphic process, and arrived at the following orbit, which scarcely differs from my previous one:—

Perihelion Passage	1836·40
Inclination	27° 36′
Position of Ascending Node	19 7
Angle between Lines of Nodes and Apsides	295 13
Excentricity	0·8794
Period in Years	184·53

It is extremely desirable that all observers of double stars should devote the most careful and unremitting attention to this star; and if this be the case, I believe, as Capt. Smyth remarked in a letter to me, "The epoch of 1857 will find the excentricity, inclination, perihelion passage, and period, all in unison." It is very satisfactory to find how closely my result for 1852 corresponds with those of Mr. Dawes and Mr. Miller, all being taken independently and without concert. I subjoin the three results:—

Miller	1852·26	Position	175°·28	Distance	3″·122
Dawes	1852·32	—	175·27	—	3·016
Fletcher	1852·42	—	175·40	—	3·147

ζ Herculis. Σ 2084.

Of late years this extraordinary star has become comparatively of easy measurement in *position*, and I have considerable confidence in my results. As its period is probably shorter than that of any other known binary star, it is very desirable that it should be well watched *now* when it is within the reach of ordinary telescopes. I have computed the elements of its orbit, by Sir John Herschel's method, and have obtained the following results:—

Perihelion Passage	1830·56
Inclination	140° 39′
Position of Node	217 14
Angle between Lines of Nodes and Apsides	266 53
Excentricity	0·4381
Period in Years	37·21

The period in this case, as well as in that of *γ Virginis*, was obtained by cutting the ellipse out of card-board, and weighing the different portions of it in a delicate balance. Mr. Bishop, in his invaluable volume of observations, has pointed out how enormously in some instances, the angles obtained by different observers about the same epoch differ from each other. It is, therefore, satisfactory to find recent observations agreeing more closely, as will be seen from the following comparison of observations for 1852 :—

Fletcher	1852·64	Position 83°·97	Distance 1″·242
Dawes	1852·73	— 82·52	— 1·567
Miller	1852·77	— 84·07	

Mr. Fletcher's paper concludes with a summary of the results of a series of double-star operations carried on in the years 1851 to 1853 at Whitehaven, by J. F. Miller, Esq., F.R.S. These results all relate to objects included in Mr. Fletcher's catalogue, and they have been obtained by an instrument of the same size as Mr. Fletcher's, though differently mounted. It is also the workmanship of Mr. Cooke.

Micrometrical Measures of ξ Ursæ Majoris, made at the Observatory, Whitehaven. By J. F. Miller, Esq. F.R.S.

Magnitudes, 4½, 5. Both stars white.

No.	Position.	Weight.	Obs.	Distance.	Weight.	Obs.*	Epoch.
	° ′			″			
19	119 36	3	8	2·962	2	8	1853·082
21	118 46	2	8	2·991	4	8	·189
22	118 7	4	8	2·974	4	8	·191
31	118 52	4	8	3·097	4	8	·191
Means	118 47	13	32	3·012	14	32	1853·190
Means	122 17	20	57	2·898	18	52	1852·129

Notes on the Experiences of W. S. Jacob, Esq., H. E. I. Co.'s Astronomer, with the new Madras Equatoreal. By Professor Piazzi Smyth.

Having been favoured in my private correspondence with Mr. Jacob during the last year with many particulars of his observations beyond those contained in his recently communicated paper to the Royal Astronomical Society, and as some of these particulars have a high importance when taken in connexion with the observations of other astronomers, recently published in the *Monthly Notices*, and others again are highly worthy of the at-

* "The distances represent double measures, consequently each set really consists of sixteen observations."

tention of good observers during the ensuing season, an immediate publication appears desirable.

The instrument employed was an equatoreal, recently added to the establishment of the Madras Observatory by the liberality of the Hon. East India Company, and carrying a telescope with a 6-inch object-glass, of 7·5 feet focus; the magnifying power most frequently used was 270, Mr. Jacob always preferring as low as possible compatible with the matter on hand. The polar axis was of the long English form, with the telescope in the middle, on a short double transit-axis; everything being of metal, by MM. Lerebours and Secretan of Paris.

The original order for the instrument was given several years since at a time when it appeared to be thought by the practice of private observers, as well as several public ones, that good telescopes were only procurable at Munich. But in addition to disapproving of the principle of the Munich form of mounting, both Mr. Jacob and myself, to whom he intrusted the order, at that time a private one, had a repugnance to lend any countenance to a monopoly, kept up, generation after generation, not so much by the fair means of superior skill and workmanship as by the unfair means of the conservation of a secret of manipulation. Secrets are radically opposed to the principles of science and the practice of scientific men; and opticians who, in making an object-glass, are profiting by the thousand freely published discoveries of scientific men, are not acting rightly in keeping secret their own single discovery. They even, thereby, retard the improvement of the telescope; for there can be no doubt that were the Munich secret published, it would soon be improved and enhanced by the varied genius and ideas of all other men, just as Daguerre's discovery was so greatly advanced within a few months after its publication, though he had brought it to the ultimate perfection of his own mind, and, as he erroneously thought, the final completion of the thing itself. Even in ordinary trade and manufactures, secretiveness is exploded everywhere, except, perhaps, amongst the most ignorant of uneducated workmen; and a free interchange of ideas as well as commodities is recognised as the surest means of advancement. By no means, then, can concealment be allowed to predominate in the most scientific of all manufactures, and my friend and myself accordingly determined, at least, to try whether a first-rate object-glass could not be procured anywhere else than at Munich.

Attracted to MM. Lerebours and Secretan,* both by much previous fame, as well as by the lowness of their prices, we found all their instrumental work of the first quality; and though the first object-glass failed, the second, which had been tested at the Edin-

* At the time of the order being given, 1847, it was believed that a sufficiently good telescope could not be made in this country, and that, perhaps, was the reason why the Liverpool observatory was furnished with a Munich object-glass. Now, however, matters are improved, and several makers have distinguished themselves, Mr. Cooke, of York, perhaps chiefly, judging from published observations.

urgh observatory previously to being sent out to India, has proved qual, if not superior, to anything which could have been expected, rom the published performances of telescopes of equal size by ther makers.

An approximate trial of the new object-glass was made by Mr. acob on its arrival at Madras in August 1852, on several double tars with the following results:—

λ *Ophiuchi* divided by one diameter of B.

ε *Ophiuchi* clearly divided.

Antares, companion immediately discovered; its previous detection in America not being then known in Madras.

μ *Boötis* elongated.

ζ *Boötis* well divided.

B of γ *Andromedæ*, sufficiently elongated to get a fair measure.

ω *Leonis* elongated.

No great importance is to be attached to these observations, as hey were hastily made without waiting for any nights of remarkbly good definition; and I have no recent measures of the objects y other observers to compare them with; but, on reference to my ather's cycle, it will be seen that the object-glass may certainly ank amongst the first, and go forward to a further trial.

Such a one is presented by certain features of the planet *Saturn*, hich has been an object of constant observation to some of our est telescopes and most practised observers during the past winter. A planet in general is certainly no accurate test of defining power, nless there be some special test-objects upon it; and the fine diision of the outer bright ring of *Saturn* is exactly this species of roof; and as difficult for the telescope as the fine markings on the 'navicula" for the achromatic microscope.

Such a fine division has been from time to time suspected by everal observers, but was always so faint, and seen through such a mall portion of the circumference, and at so very few and such listant intervals of time, that where its existence was believed in at ll, it was thought to be temporary, or at all events to vary in idth and consequent visibility. If this be true, the same epoch of ime must be employed in the comparison of telescopes, and I limit yself, therefore, to the observations of last winter.

The best 6-inch object-glass that Munich has ever produced is robably that in the possession of the Rev. W. R. Dawes, probably lso our best observer. By his letters in the *Monthly Notices*, luring the last winter, he appears to have had a faint suspicion of races of the fine division about the ansæ, but has not subsequently nsisted upon it. At the same time, Mr. Lassell, with his splendid eflecting telescope, with its mirrors never better, in the clear climate f Malta, and with the planet at a greater height, represents himelf as certain that there is no such division. But at the very same ime, Mr. Jacob, with *Saturn* certainly a few degrees higher, after ery careful adjustment of the focus, immediately saw the fine

division, and continued to see it during the seven months of the planet's apparition, on every night he looked, except one or two of bad definitions; with powers varying from 170 to 370. It was seen too, moreover, through more than half the circumference of the ring, and with abundant certainty to admit of micrometer measurement.

This certainly appears the most satisfactory account we have ever had of the fine division, especially as the rings are not yet at their maximum inclination; and while it indicates that the notion of the variable breadth of the separation may have been too freely drawn upon, it certainly proves Lerebours' object-glass to give much better definition than Mr. Lassell's speculum, and probably better than Mr. Dawes' Munich object-glass, unless a very wide margin be left for the effect of the different altitudes of the planet at the two places.

Amongst many other features in the appearance of the planet, mentioned by Mr. Jacob, are the transparency of the dark ring, and its brown colour by the transmitted light of the planet, which are both mentioned in a letter, dated Madras, Oct. 10, 1852, and referring to the appearance of the planet on Sept. 22, 1852. He also mentions a suspicion of a sort of scalloping, like the fold of a curtain, on the inner ansæ of the dark ring, and finds the principal division of the bright ring to be filled with a half-luminous medium, and shaded off so much on one edge as to make the accurate measure of its breadth very doubtful.

There can, however, be no doubt of the glasses of the telescope being very transparent, as well as defining well.

Mr. Jacob having lately commenced the regular observation of a selection of double stars, with a view to parallax, though with little hope of success; inasmuch as though this was the final object in view, for which they were taken up by Sir W. Herschel and other observers, no one has yet succeeded in finding the desired quantity. Mr. Jacob was, therefore, unexpectedly pleased at finding, towards the end of last year, that the components of *α Herculis* showed evident symptoms of parallax.

[Professor Smyth then gives the details of Mr. Jacob's observations, which, being similar to those mentioned at page 242, need not be repeated here. He concludes his paper by stating that, as the components of *α Herculis* form one of the most remarkable pairs in the sky for contrasted colours, they may thereby serve to indicate the colour of the medium pervading space.]

On the Corrections to be applied to Burckhardt's and Plana's Parallax of the Moon, expressed in terms of the Mean Arguments. By J. C. Adams, Esq., M.A.

In the Supplement to the *Nautical Almanac for* 1856, I have given new tables of the moon's parallax, adapted to Burckhardt's

form of the arguments. When the arguments have been already computed, these tables supply the most convenient means of finding the parallax, and they have, accordingly, been used in calculating the corrections to the *Nautical Almanac* Parallaxes since 1840, given in the paper above referred to.

When, however, Burckhardt's arguments are not previously known, it will be more simple to employ arguments increasing proportionably to the time, in order to calculate either the parallax itself immediately, or the correction to be applied to that found from Burckhardt's tables.

The following formulæ may be used for this purpose, the arguments being expressed in Damoiseau's notation.

The moon's equatoreal horizontal parallax, or, more strictly, the *sine* of that quantity converted into seconds of arc is equal to

$$
\begin{aligned}
&3422''\cdot32 + 186\cdot51 \cos x + 10''\cdot17 \cos 2x + 0''\cdot63 \cos 3x + 0''\cdot04 \cos 4x \\
&-0''\cdot95 \cos t + 28''\cdot23 \cos 2t + 0''\cdot26 \cos 4t \\
&\qquad + 34''\cdot30 \cos (2t - x) + 0''\cdot37 \cos (4t - 2x) \\
&-0''\cdot40 \cos z + 1''\cdot92 \cos (2t - z) + 1''\cdot45 \cos (2t - x - z) \\
&+1''\cdot16 \cos (x - z) - 0''\cdot71 \cos (2y - x) - 0''\cdot95 \cos (x + z) \\
&+0''\cdot01 \cos (x - t) - 0''\cdot31 \cos (2x - 2t) \\
&-0''\cdot31 \cos (2t + z) - 0\cdot23 \cos (2t - x + z) \\
&-0''\cdot11 \cos (2y - 2t) + 0''\cdot22 \cos (2t + x - z) - 0''\cdot12 \cos (3x - 2t) \\
&+0''\cdot14 \cos (t + z) + 3''\cdot09 \cos (2t + x) + 0''\cdot60 \cos (4t - x) \\
&-0''\cdot11 \cos (t + x) + 0''\cdot28 \cos (2t + 2x) \\
&+0''\cdot12 \cos (2x - z) - 0''\cdot10 \cos (2x + z) + 0''\cdot09 \cos (2t - 2z) \\
&-0''\cdot09 \cos (2y + x - 2t) + 0''\cdot05 \cos (2t - x - 2z) \\
&+0''\cdot06 \cos (4t - x - z).
\end{aligned}
$$

Also, the correction to be applied to the equatoreal horizontal parallax found from Burckhardt's tables is

$$
\begin{aligned}
&1''\cdot79 + 0''\cdot13 \cos x + 0''\cdot06 \cos 2x + 0''\cdot14 \cos 3x + 0''\cdot04 \cos 4x \\
&+0''\cdot06 \cos t + 0''\cdot05 \cos 2t - 0''\cdot29 \cos 3t + 0''\cdot17 \cos 4t \\
&\qquad - 0''\cdot18 \cos (2t - x) + 0''\cdot01 \cos (4t - 2x) \\
&+0''\cdot05 \cos z + 0''\cdot93 \cos (2t - z) + 1''\cdot15 \cos (2t - x - z) \\
&+0''\cdot07 \cos (x - z) - 1\cdot50 \cos (2y - x) \\
&-0''\cdot05 \cos (x - t) + 0''\cdot02 \cos (2x - 2t) \\
&-0''\cdot90 \cos (2t + z) - 1''\cdot17 \cos (2t - x + z) \\
&-0''\cdot12 \cos (2y - 2t) + 0''\cdot12 \cos (2t + x - z) + 0''\cdot10 \cos (3x - 2t) \\
&+0''\cdot14 \cos (t + z) + 0''\cdot09 \cos (2t - 2z) - 0''\cdot06 \cos (2y + x - 2t) \\
&+0''\cdot05 \cos (2t - x - 2z) + 0''\cdot07 \cos (2x + z - 2t) \\
&-0''\cdot09 \cos (2t + x - 2y)
\end{aligned}
$$

In both the above formulæ, quantities less than $0''\cdot05$ have been neglected, except where they can be included in the same table with larger terms.

When Burckhardt's parallax is known, it will be sufficient for ordinary purposes to calculate the correction to be applied to it, taking into account only the constant term, and the periodic terms depending on the arguments,

$$x,\ l,\ 2l-x,\ 2l-z,\ 2l+z,\ 2l-x-z,\ 2l-x+z,\ 2y-x,\ l+z.$$

If extreme accuracy be required, the parallax should be calculated afresh by means of the first of the above formulæ.

These formulæ, as well as my tables in the Supplement to the *Nautical Almanac for* 1856, give the value of the *sine* of the parallax, converted into seconds of arc, which is frequently more convenient for use than the parallax itself.

To find this latter quantity, we must add

$$0''\cdot16 + 0''\cdot03 \cos x.$$

Plana's formula for the parallax, as given in the Introduction to the Greenwich Lunar Reductions, also requires several corrections, partly in consequence of the developements not having been carried far enough, and partly from errors in the numerical conversion of the analytical expression.

The constant of parallax employed in the Lunar Reductions appears to be Henderson's, or $3421''\cdot8$; and the computed quantity is taken to be the parallax itself.

The correction to be applied to the parallax thus found, in order to make it agree with my determination, is given by the following formula:—

$$\begin{aligned}
&0''\cdot68 - 0''\cdot16\cos x - 0''\cdot13\cos 2x + 0''\cdot03\cos 3x + 0''\cdot04\cos 4x\\
&-0''\cdot05\cos l + 0''\cdot63\cos 2l + 0''\cdot16\cos 4l\\
&+0''\cdot40\cos(2l-x) + 0''\cdot07\cos(4l-2x)\\
&-0''\cdot28\cos(2l-z) - 1''\cdot91\cos(2y-x) + 0''\cdot29\cos(2l+z)\\
&+0''\cdot01\cos(x-l) - 0''\cdot51\cos(2x-2l)\\
&+0''\cdot09\cos(2y-2l) + 0''\cdot14\cos(l+z) + 0''\cdot10\cos(4l-x)\\
&-0''\cdot11\cos(l+x) - 0''\cdot02\cos(2l+2x)\\
&+0''\cdot12\cos(2x-z) - 0''\cdot10\cos(2x+z) + 0''\cdot09\cos(2l-2z)\\
&-0''\cdot09\cos(2y+x-2l) + 0''\cdot05\cos(2l-x-2z)\\
&+0''\cdot06\cos(4l-x-z)
\end{aligned}$$

As before, quantities less than $0''\cdot05$ have been neglected, except when they unite with larger terms.

In the American *Nautical Almanac for* 1855, recently published, Plana's formula for the parallax appears to have been employed; the constant, however, being slightly altered.

The following table, which Mr. Farley has obligingly calculated at my request, shows the corrections to be applied to the parallaxes given in this work, in order to make them agree with those found from my tables.

ifferences of Moon's Hor. Par. as given in the American N. A. from that obtained from Mr. Adams' Tables.

1855. G. M. Noon of each Day.

y h.	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
	″	″	″	″	″	″	″	″	″	″	″	″
	−1·5	−3·1	−2·2	−0·6	+0·8	+1·6	+1·4	−0·2	−2·3	−2·6	−3·3	−1·3
	1·7	3·4	2·2	0·6	0·8	1·2	0·9	1·1	2·9	2·9	3·3	0·9
	2·4	3·4	2·2	0·8	0·4	0·8	+0·2	1·9	3·6	3·2	3·1	−0·5
	2·9	3·9	2·6	1·0	+0·2	0·3	−0·5	2·6	3·9	3·7	2·4	+0·1
	3·3	4·0	2·8	1·2	0·0	+0·1	1·0	3·0	4·1	3·5	1·7	0·5
	3·5	3·8	2·7	1·1	−0·2	−0·1	1·6	3·6	4·0	3·3	1·1	0·7
	3·6	3·3	2·5	0·8	+0·2	0·2	1·8	3·8	3·8	2·8	−0·4	1·4
	3·3	2·6	2·2	−0·2	0·5	0·4	2·1	3·8	3·3	2·0	+0·5	2·1
	3·3	2·0	1·8	+0·5	0·8	0·6	2·4	3·8	2·8	1·2	1·0	2·2
	3·0	1·5	1·1	1·1	0·8	0·7	2·5	3·4	2·3	−0·4	1·5	2·2
	2·5	1·2	−0·4	1·3	0·7	0·9	2·5	3·1	1·8	+0·4	1·6	1·5
	2·2	0·8	0·0	1·3	0·6	1·2	2·5	3·0	1·2	0·7	1·4	+0·6
	1·8	−0·2	+0·4	1·4	0·5	1·5	2·6	2·7	0·6	0·8	0·8	−0·5
	1·2	+0·4	0·8	1·2	+0·3	2·0	2·9	2·4	−0·2	0·9	+0·3	0·8
	−0·4	1·1	1·2	1·2	0·0	2·4	2·9	2·0	+0·1	0·9	−0·2	1·1
	+0·5	1·7	1·3	0·8	−0·6	2·7	3·0	1·7	0·3	0·8	0·6	1·7
	1·1	1·8	1·5	+0·4	1·2	3·0	2·9	1·3	0·5	0·7	0·9	2·1
	1·3	1·2	1·6	−0·3	1·9	3·0	2·6	0·8	0·5	0·4	1·4	2·3
	1·2	+0·5	0·9	1·0	2·5	3·0	2·0	0·4	0·4	+0·1	1·7	2·6
	0·7	−0·4	+0·5	1·9	2·8	2·7	1·8	0·4	0·1	−0·5	2·2	3·0
	+0·1	1·2	−0·4	2·4	2·8	2·4	1·5	0·0	+0·1	1·0	2·4	3·1
	−0·4	1·9	1·2	2·7	2·7	2·1	0·9	−0·1	−0·2	1·4	2·3	2·5
	1·0	2·4	1·8	2·7	2·3	1·5	0·4	+0·3	0·1	1·3	2·2	2·0
	1·3	2·8	1·9	2·5	2·0	1·0	−0·1	0·6	0·0	1·3	1·7	1·5
	1·5	2·8	2·1	2·2	1·6	−0·2	+0·4	0·9	0·0	1·0	1·3	1·4
	1·7	2·7	2·5	1·8	1·1	+0·6	1·1	1·3	0·1	1·1	0·8	1·2
	1·8	2·5	2·5	1·2	−0·3	1·0	1·8	1·3	0·5	1·3	0·9	0·6
	2·2	−2·5	2·1	0·7	+0·5	1·9	1·9	+0·7	0·9	1·5	1·3	0·9
	2·3	...	1·7	−0·1	1·5	2·4	2·1	0·0	1·4	1·7	1·2	−0·5
	2·6	...	1·4	+0·4	1·9	+2·1	1·6	−0·7	−1·9	2·4	−1·4	0·0
	−2·8	...	−1·1	...	+1·9	...	+0·7	−1·6	...	−2·7	...	+0·8

The constant employed in the computations of the American *utical Almanac* does not appear to be mentioned in the Preface. may, however, be determined in the following manner:—

The sum of the daily corrections given in the above table is ·370″·4. Now, I find that −14″·1 of this is due to the corrections plied to the periodic terms, leaving −356″·3 as the effect caused

by the difference of the constants. This, divided by 365, gives −0″·98 as the correction to be applied to the constant of the American *Nautical Almanac*, in order to make it agree with my own. Hence, this latter value being 3422″·32, it follows that the constant employed in the above work is 3423″·30.

Report of the Astronomer Royal to the Board of Visitors. Read at the Annual Visitation of the Royal Observatory, Greenwich, 1853, June 4.

In this Report the Astronomer Royal exhibits the state of the Observatory on 1853, May 22, and records generally the principal occurrences from 1852, May 18, to 1853, May 22.

Under the head of *Grounds and Buildings*, an account is given of the progress of operations for forming a galvanic communication between the Observatory and other places of importance, both in this country and on the Continent. The communication with the various parts of the British Isles is maintained by means of wires passing from the Observatory to the South Eastern Railway Terminus at London Bridge. With respect to the Continent, this is effected by forming a connexion between the Observatory and the wires of the Submarine Telegraph Company, which pass underground from London to Dover. One of the Company's wires was cut where it crosses Blackheath, and the interrupted segments were continued by branches to a turn-plate, which is contained in an iron box fixed in the south wall of the Park. From this turn-plate a wire is led into the transit circle-room of the Royal Observatory. By different adjustments of the turn-plate, communications are made between London and Paris, between London and Greenwich, or between Greenwich and Paris. This part of the Report concludes with the statement that the Observatory now possesses the means of communicating with every part of Great Britain and of the Continent.

The Moveable Property, the Manuscripts, and the Library of the Observatory, are next noticed in succession. With respect to the Manuscripts, it is stated that Professor De Morgan has recently transmitted to the Observatory a considerable number of letters found in the collection of correspondence of the late Mr. Baily. These letters are, doubtless, calculated to throw much valuable light upon the scientific history of the age in which Mr. Baily lived; and on this account they cannot fail to be hereafter consulted with advantage.

Astronomical Instruments.—The condition of the various instruments is noticed under this head, and appears to be satisfactory. With respect to the recording of transits by the aid of galvanism, the following statement is made:—"The Barrel Apparatus for the American method of observing transits is not yet brought into use. In an apparatus, embodying much that is new (for the

circumstances under which we have to try it differ much from those of the original experiments) there must be a great many small alterations; and sometimes from the demand for workmen competent to perform work of this kind, a single alteration has caused more than a month's delay. I have, however, brought it to such a state that I am beginning to try whether the barrel moves with sufficient uniformity to be itself used as the transit clock. This, if perfectly secured, would be a very great convenience; but I am not very sanguine on that point. Much convenience will, however, be gained by making the barrel to move with a speed approaching very near to uniformity, even though the barrel-clock be not quite accurate enough to be used as the best measurer of time in the Observatory, and, therefore, not accurate enough to be used for impressing the second-dots upon the barrel."

Observations.—No alteration has been made in the general system of meridian observations. The observation of the moon on the meridian is never omitted; that of the sun and planets only on Sundays. The whole number of observations from 1852, May 18, to 1853, May 22, is nearly as follows:—In the transit department: transits, 4037; observations of collimators, 312; observations of transit wire by reflexion, 312; observations of one collimator by the other, 52. In the meridian circle department: observations of all kinds, 4475.

The Reflex Zenith Tube has been in active use during the past year. Between May 18 and October 20, 106 single observations have been obtained with it.

The Altazimuth has been used in precisely the same manner as in the preceding years. The number of days of complete observations of the moon is 224, or 18 per lunation, against 106 with the transit-circle, or $8\frac{1}{2}$ per lunation. Of the altazimuth observations, 0 are when the moon passed between 0^h and 1^h, 4 between 1^h and 2^h, 8 between 2^h and 3^h, 8 between 3^h and 4^h, 6 between 20^h and 21^h, 0 between 21^h and 22^h, 1 between 22^h and 23^h, 1 between 23^h and 24^h. In none of these positions, except on a single occasion between 20^h and 21^h, was the moon observed on the meridian. The whole number of separate observations of moon and stars with the Altazimuth is 1003; and the whole number of separate observations of its collimator, 712.

The South Equatoreal has been used only for images of the solar spots. The Double-Image Micrometer has been employed in executing measures of *Venus*, *Saturn* and his Rings, and *γ Virginis*.

The Reduction and Printing of the astronomical observations are next noticed in the Report. The advanced state of the Reductions (although not so close as usual, in consequence of a serious diminution of the strength of the establishment arising from the illness of several of the assistants) is attributed chiefly to the order and energy of Mr. Main. Allusion is then made to the operations connected with the Magnetical and Meteorological departments of the Observatory.

Chronometers, Communications of Time, and Operations for Longitude. — Under this head various interesting facts are detailed. "The number of chronometers now on hand," it is stated, "is 120. The system of rating the chronometers daily or weekly, as the case appears to require, of reporting the rates to the Admiralty, of rating the trial-chronometers in extremes of heat and cold, of abstracting their rates, of superintending repairs, and of dropping the signal-ball, remains unaltered. In the mechanical part of the operation of rating, an alteration has been made which contributes greatly to convenience, and in some measure to accuracy. At the time of the Visitors' last meeting, a normal clock had been erected by Mr. Shepherd, furnished with a small apparatus suggested by myself (an auxiliary pendulum, which can be made very long or very short, and can in either state be connected with the clock-pendulum), by means of which the indication of the clock can be increased or diminished by any required quantity above $0^s{\cdot}01$. The error of this clock being ascertained every day, by means of another clock close to its side, which has been compared with the transit-clock, there is no difficulty (with the use of the auxiliary apparatus above-mentioned) in making it sensibly correct. This clock keeps in motion a sympathetic galvanic clock in the chronometer-room, which, therefore, is sensibly correct; and thus the chronometers are compared with a clock which requires no numerical correction. I need not insist on the facility, and the freedom from a fruitful source of errors, which are thus obtained.

"The same normal clock maintains in sympathetic movement the large clock at the entrance-gate, two other clocks in the Observatory, and a clock at the London Bridge Terminus of the South-Eastern Railway (first tried with the assistance of C. V. Walker, Esq., as an experiment, but now to be used for automatically making and unmaking certain connexions of our galvanic wires). It sends galvanic signals every day along all the principal railways diverging from London. It drops the Greenwich ball, and the ball on the offices of the Electric Telegraph Company in the Strand; and the correctness of the last of these operations is tested by means of a galvanic signal-needle upon the case of our transit-clock. All these various effects are produced without sensible error of time; and I cannot but feel a satisfaction in thinking that the Royal Observatory is thus quietly contributing to the punctuality of business through a large portion of this busy country.

"I have the satisfaction of stating to the Visitors that the Lords Commissioners of the Admiralty have decided on the erection of a time-signal-ball at Deal, for the use of the shipping in the Downs, to be dropped every day by a galvanic current from the Royal Observatory. The construction of the apparatus is intrusted to me. Perhaps there is no roadstead in the world in which the knowledge of true time is so important."

The operations for the determination of longitudes by means of electric signals are then referred to. The details with respect to

Cambridge and Edinburgh have been already mentioned in the lecture of the Astronomer Royal at the last meeting of the Society.* With respect to the Oxford Observatory, Mr. Airy stated that the wires for connecting the Observatory with the Railway Station at Oxford are nearly completed, so that the determination of the longitude of the Observatory by galvanic signals may be expected at no distant date.

The galvanic determination of the longitude of the Royal Observatory of Paris and of other Continental observatories relative to Greenwich, will constitute the natural sequel to these operations.

After a brief allusion to the personal establishment of the Observatory, the author of the Report proceeds to notice the "*Extraneous Computations.*" Under this head it is stated that the revision of the Star Results of Dr. Maskelyne is now completed, and a hope is justly expressed that "it will be found that the publication of these deductions will give most valuable information on the state of the sidereal heavens from 1765 to 1807."

A proposal is next made respecting the Observations of the Moon. The great work on the Lunar Reductions terminates with the year 1830. The theory employed in computing the tabular places of the moon, with which the results of observation were compared, was mainly that of Plana. In recent years (from the beginning of 1836) the places of the moon deduced from the observations have been compared with the corresponding places in the *Nautical Almanac*, calculated from Burckhardt's Tables; but, as the latter have been found by Mr. Adams to be sensibly erroneous, in so far as regards the parallax, the Astronomer Royal suggests whether it would not be desirable at once to complete the reduction of the observations and their comparison with tables, from 1831 to 1851; using, as far as possible, in all respects the same bases of reduction of observations and the same elements of tables as in the former Lunar Reductions.

The Report concludes with some general remarks relative to the present satisfactory condition of the Observatory.

Astronomische Beobachtungen auf der Sternwarte zu Bonn augestellt und herausgegeben von Dr. F. W. A. Argelander. Zweiter Bund, Zonenbeobachtungen am Südhimmel. Zweiter Abtheilung. 4to. pp. 427. Bonn, 1852.

This volume forms the second part of M. Argelander's zone observations included between 15° and 31° of south declination. An account of the observations contained in the first part, including a statement of the general plan of the work, is given at page 26 of the present volume of the *Monthly Notices.* The zones in the part which has recently been published, as mentioned above, extend from No. 322 to No. 404.

* See p. 248.

Astronomical Observations made at the Radcliffe Observatory in the Year 1851. By Manuel J. Johnson, M.A., Radcliffe Observer. Vol. XII. 8vo. Oxford.

This volume contains the usual class of observations with the meridian instruments. These are followed by a catalogue of circumpolar stars observed in the year 1851. The number of stars is 2243. Mr. Johnson has not yet completed the entire series of observations for the Circumpolar Catalogue, as a considerable number of objects not included in the Catalogue of Groombridge require further revision. In the Circumpolar Catalogue of the present volume there will be found more than the usual number of stars within 5° or 6° of the pole. Mr. Johnson has for some years past bestowed great attention on the stars situate in that region of the heavens. The principal object which he had in view has been to supply ready means for determining the meridian error of the transit instrument, by observations of two such stars at their opposite culminations; a method which, after long trial, he recommends for its convenience and accuracy.

There are two Appendixes to this volume. Appendix I. contains "Remarks on the Application of the Heliometer to the Photometry of the Stars." The interest attached to the subject of this paper, and the valuable character of the results, render a separate account of it desirable, which will be found in a Supplement to the present *Notice*. Appendix II. consists of an Index, showing the volumes of the Radcliffe Observations, vol. i.–xii., in which observations of stars in Groombridge's Catalogue will be found.

A Supplement to No. 864 of the *Astronomische Nachrichten* contains a biographical notice of the late Professor Schumacher by M. Olufsen, accompanied with a lithographic portrait of the revered astronomer, and a fac-simile of his handwriting.

*** *Several Communications have been omitted in the present Number for want of space. These will shortly be published in a Supplement, which will also contain the Index and other particulars relating to the completion of* volume xiii. of the *Monthly Notices.*

London:—Printed by G. Barclay, Castle St. Leicester Sq.

Note on the small Star near α Canis Minoris, and on the suspected variable Star β Corvi. By the Rev. T. W. Webb.

"Mr. Fletcher's very interesting remarks on the companion of *α Canis Minoris* having induced me to refer to some former observations of my own, I find that I saw it on February 28, 1848, with a power of 80, in my 5½-feet achromatic. As Mr. Fletcher states that Mr. Bond announced its disappearance *in the same year*, this circumstance would seem to indicate a rapid variation o brightness, especially when the great disparity of the instruments is taken into consideration, my achromatic having an aperture of only 3·7 inches. At the time I saw it, it was probably not much fainter than the 8 or 9 magnitude, as I have not noted any discrepancy in that respect from the Bedford Catalogue, which I should have done, had there been any very striking diminution.

"I have observed the comparative brightness of the stars in *Corvus*, referred to in page 186 of the *Monthly Notices* for the present year, on one occasion only during the late spring. May 23, 1853, a little after 9^h, in twilight so strong that *Spica* alone, in all that region, was visible to my near-sighted eye, and with a concave eye-glass the principal stars in *Corvus* could hardly be well seen, unless brought near the centre of the field of view, I made an independent estimate of their magnitude without being able to recollect their alphabetical designation. A subsequent reference to the map showed that I had arranged them thus:—

$$\gamma\ \beta\ \delta\ \alpha,$$

with some doubt whether α might not have rivalled δ, at an equal elevation. It will be seen that this estimate differs from that of the spring of 1852, in which I gave δ the preference to β. But they both agree in giving the priority to γ, and increasing the suspicion that β has diminished since 1831. The latter observation may probably be entitled to most confidence, as, for obvious reasons, slight differences in brightness are most apparent near the limit of visibility;—a fact which, by the way, may give an advantage to the smaller class of telescopes, in examining the comparative light of stars between the 10, and 12, or 13 magnitudes."

"*Tretire, Herefordshire, June* 22, 1853."

The name of *Proserpine* has been given, at the suggestion of M. Humboldt, to the planet discovered by M. Luther on the 5th of May last.

Observations of Comet II., 1853 (*Schweizer's*). By Lieut. Goodenough, R.N.

(*Extract of a Letter to the Rev. Robert Main.*)

"I regret very much having sent you last month the observations of a comet seen here on April 30, as I immediately afterwards discovered, that from want of caution I had set down and worked out the degree of the distance from *Castor* as 58° instead of 63°, which was the correct. It is not possible to hold myself excused for so stupid an error. Allow me, however, to send to you the following set of sights, observed with one of Troughton's sextants, and most of them under favourable circumstances, the atmosphere of this place being generally peculiarly clear and free from haze.

April 30.

	h m s / ° ′ ″
Greenwich M.T.	11 9 37
True dist. from Castor	63 28 2
— — — Sirius.....	34 43 49

which give,

R.A. 4 h 16 m 10 s Decl. 12° 15′ 7″ S

May 1.

	h m s / ° ′ ″
Greenwich M.T.	11 32 2
True dist. from Rigel	5 12 30

No other observation could be obtained on account of clouds.

May 2.

	h m s / ° ′ ″
Greenwich M.T.	12 9 43
True dist. from Sirius	14 31 11
— — — Canopus...	39 27 26

which give,

R.A. 5 h 39 m 19 s Decl. 14° 2′ 0″ S

May 3.

	h m s / ° ′ ″
Greenwich M.T.	10 50 26
True dist. from Regulus ...	65 24 50
— — — Bellatrix ..	22 57 37

R.A. 5 h 59 m 41 s Decl. 14° 11′ 30″ S

May 4.

	h m s / ° ′ ″
Greenwich M.T.	10 37 30
True dist. from Regulus ...	61 56 24
— — — Betelguese	22 41 10

R.A. 6 h 14 m 57 s Decl. 14° 14′ 31″ S

May 5.

	h m s / ° ′ ″
Greenwich M.T.	10 48 11
True dist. from Regulus ...	59 20 37
— — — Betelguese	23 44 54

R.A. 6 h 26 m 57 s Decl. 14° 13′ 54″ S

May 6.

	h m s / ° ′ ″
Greenwich M.T.	10 47 55
True dist. from Regulus ...	57 23 14
— — — Pollux	44 59 20

R.A. 6 h 35 m 42 s Decl. 14° 9′ 16″ S

May 7. Obscured by cloud.

May 8.

	h m s / ° ′ ″
Greenwich M.T.	11 0 19
True dist. from Procyon...	22 23 7
— — — η Argus ...	61 39 41

R.A. 6 h 48 m 14 s Decl. 14° 2′ 6″ S

May 9.

	h m s / ° ′ ″
Greenwich M.T.	10 44 59
True dist. from Procyon...	21 48 40
— — — Betelguese	26 51 17

R.A. 6 h 52 m 49 s Decl. 13° 58′ 55″ S

May 10 *and* 11. Too cloudy.

May 12.

	h m s / ° ′ ″
Greenwich M.T.	10 35 0
True dist. from Procyon...	20 41 17
— — — Betelguese	28 11 22

R.A. 7 h 3 m 30 s Decl. 13° 30′ 21″ S

"On April 30, when first seen, the tail was about 4° in length, the nucleus very distinct from the coma, of the brightness of a star between 3d and 4th mag. On May 2 the *distinct* part of the tail had diminished to 1° 30′ in length, the brightness of the nucleus having diminished to that of a star of the 6th mag. It afterwards kept diminishing, and was last seen on the 17th, only by the aid of a good 2-foot telescope, of about 20 magnifying power, in a perfectly clear sky. Between the 12th and 17th the nights were cloudy.

"The distances have all been carefully corrected for refraction.

"Our position is, lat. 34° 32′ 30″ S; long. 58° 18′ 0″ W.

"*H.M.S. Centaur, Buenos Ayres,*
May 21, 1853."

(*Extract of a Letter from A. H. Gardner, Esq., Commander of H.M. Sloop Waterwitch, to Rear-Admiral Sir Francis Beaufort.*)

"When approaching Loando in her Majesty's sloop, under my command, on the 30th April, a comet was seen clearly by the naked eye, continued visible during the passage over to St. Helena, and was last seen near that island on the 14th inst. It was also seen, but not accurately observed, at St. Helena, as well as on board several merchant vessels. As no notice of its approach is given in the *Nautical Almanac*, and as it could not be seen in our country, I have thought an account of it might be interesting to you. The inclosed paper is by Mr. Vine, master's assistant, who first perceived the comet.

"*H.M. Sloop Waterwitch, Ascension, May* 31, 1853."

Observations of Comet II. 1853. By Mr. W. W. Vine.

"On April 30, 1853, about 7 p.m., I observed a comet bearing W. by S. (true), about 10° above the horizon. Its distance from *Sirius*, 37° 41′ 15″; from *Aldebaran*, 28° 38′ 0″. Its tail was 3° 15′ long, and pointed in the direction of *Canopus*. Our latitude at the time of observation was 8° 40′ S.; longitude, 12° 38′ E.

"On May 1st, comet visible, but not so bright as on the previous night, but its altitude about 13° higher. Distance from *Sirius*, 24° 45′ 20″; *Canopus*, 39° 50′ 30″; tail 3° long, and pointing in the same direction as before at St. Paul's de Loando.

"On May 2d, comet much decreased in size and brightness. Distance from *Sirius*, 16° 14′ 50″; *Canopus*, 39° 50′ 30″. Latitude, 8° 40′ S.; longitude 13° 0′ E. at time of observation.

"On May 3d, distance from *Sirius*, 10° 33′ 50″; *Canopus*, 38° 24′ 30″. Visible to the naked eye. Latitude, 8° 35′ S.; longitude, 12° 40′ E.

"On May 4th, comet in the constellation of *Canis Majoris*. I saw a star of (about) the sixth magnitude through its tail distinctly. Distance from *Sirius*, 6° 38′ 30″; *Canopus*, 38° 24′ 30″. Latitude, 8° 30′ S.; longitude, 12° 0′ E.

"On May 5th, distance from *Sirius*, 3° 59′ 0″; from *β Canis Majoris*, 4° 21′ 30″. Latitude, 8° 35′ S.; longitude, 10° 30′ E.

"On May 6th, distance from *Sirius*, 2° 31′ 30″; from *β Canis Majoris*, 5° 46′ 20″. Very indistinctly seen. Latitude, 8° 20′ S.; longitude, 8° 35′ E.

"On May 7th, comet not bright enough to get its position with a sextant. It continued visible until the 15th, when we lost sight of it. We were then in latitude 13° 25′ S.; longitude, 3° 30′ W. I determined its position by right ascension and declination for fifteen nights, the time it remained visible, taking my observations at 7 P.M. (ship time). Its path through the heavens was E. ¼ S. (nearly).

Observed Right Ascension and Declination of the Comet from April 30th to May 7th, at 7 P.M. (ship time).

Date.	R.A.	Change of R.A. in 24 hours.	Decl. S.	Change of Decl. in 24 hours.
	h m s	m s	° ′ ″	° ′ ″
April 30	4 4 3	+52 29	11 50 28	+1 35 32
May 1	4 56 32	35 16	13 26 0	0 33 30
2	5 31 48	23 36	13 59 30	0 14 0
3	5 55 24	16 16	14 13 30	0 1 45
4	6 11 40	12 50	14 15 15	0 0 45
5	6 24 30	8 30	14 16 0	+0 0 30
6	6 33 0	7 0	14 16 30	No change.

Mr. Vine's observations extend to the 14th of May. They do not, however, indicate any sensible change in declination after the 6th of that month.

Observations of Comet II., 1853. By Thomas Maclear, Esq.

"After some days of clouded weather, a comet was detected with the naked eye, in right ascension $4^h\ 58^m$, north polar distance 103° 25′, on the evening of Sunday the 1st instant. The nucleus about equal in brilliancy to a star of the 4th mag. The tail about 4° in length, inclined 118° from the north, reckoned as in measuring double stars, and one-sided with respect to the nucleus.

"The nucleus fan-shaped under a high power.

"The sky was covered on the 2d. On the 3d, the right ascension was $5^h\ 59^m$, north polar distance 104° 11′; and on the 4th, $6^h\ 13^m$, and 104° 15′ respectively.

"The nucleus was still pretty bright on the 9th, though nigh invisible to the naked eye. Last night all trace of the tail had vanished, the right ascension $7^h\ 16^m\frac{1}{2}$, north polar distance 103° 12′.

"The weather has permitted seventeen observing nights out of twenty-three. The comparisons with stars near the path have been made with the spider-line micrometer attached to the 8½-foot

equatoreal; and the number of stars compared amount to 24. The details will be forwarded by the next mail.*

"There is a notice in the *Astronomische Nachrichten*, No. 853, received yesterday, of a comet detected by Professor Secchi, on the 6th of March, supposed to have passed its perihelion on the 24th of February, and to have been a conspicuous object in this hemisphere. I am not aware of any but the one above reported.

"*Royal Observatory, Cape of Good Hope,*
May 24, 1853."

The following notes and observations of Comet II., 1853, have been communicated by Capt. Charles F. A. Shadwell, R.N., who obtained possession of the materials of them during his recent passage from India to this country in the Royal Mail Steamer, Lady Jocelyn. These observations were made on the occasion of the outward passage of the Lady Jocelyn to India. The computations which assign the comet's position in right ascension and declination are due to Capt. Shadwell.

Notes on the Comet of May 1853, *observed on board the Royal Mail Steamer, Lady Jocelyn.* By Mr. S. H. Wright, Second Officer.

1853, April 30. The comet was first seen on April 30, soon after sunset, at $6^h\ 45^m$ p.m., the Lady Jocelyn being then on her passage from the Cape to Mauritius, and was thus described.

The nucleus of the comet was as large as a star of the 1st magnitude, and of a pale blue colour. The tail pointed towards *Rigel*, was about 6° in length, distended at the outer end about 20′, and of a streaky appearance.

May 1, 7 p.m. The nucleus of the comet was less in size by about one magnitude than when seen yesterday. The tail had the same appearance, but was considerably diminished in point of size, not being more than 3° in length.

May 2, $7^h\ 15^m$ p.m. The comet has again decreased in size, the tail being only 1° 30′ in length. The nucleus not larger than a star of the 3d magnitude, and very faint.

Owing to cloudy weather the comet was not seen on May 3d and 4th.

May 5, $7^h\ 45^m$ p.m. Comet again visible. Tail partly surrounding the nucleus, being 10′ to the westward and 45′ towards the eastward. The streaky appearance has left it, and it is now of a pale light.

May 6, 8 p.m. Comet getting very faint. When viewed

* The paper containing the observations here referred to has been received at the Apartments of the Society. The results will be published in the *Monthly Notice* for November.

through a telescope the nebulous part seemed completely to surround the nucleus in the form of an oval. The greatest diameter from E.S.E. to W.N.W. about 1° in length. The lesser diameter about 30′.

May 7. Comet growing fainter. Distance to comparison stars measured with difficulty.

For several days subsequent to the last observation the comet could be seen through a telescope, gradually diminishing in size, but retaining very nearly the same position in the heavens.

The following table gives the observations made between April 30th and May 7th, with stars lying near the comet's path, and the computed places resulting therefrom. The observations are uncorrected for refraction :*—

Date. 1853.	Position of Ship.	Approx. G.M.T.	Distance from Comparison Stars.		Computed R.A.	Computed Decl.
	° ′	h m		° ′	h m s	° ′
April 30	35 50 S	4 17	α Orionis ...	33 6	3 57 47	−11 28·5
	36 15 E		η Argûs	85 28		
May 1	35 13 S	4 18	Rigel	5 58	4 54 17	−13 23·5
	39 38 E		Sirius.....	25 23		
2	33 58 S	4 14	Rigel	8 4	5 31 14	−13 57·4
	44 26 E		Canopus ...	39 54		
5	28 25 S	4 5	β Can. Maj.	4 3	6 23 27	−14 12·7
	54 23 E		Sirius.....	4 20		
6	25 34 S	4 12	Canopus ...	38 44	6 29 45	−13 55·0
	56 16 E		Procyon.....	24 50		
7	22 32 S	4 9	β Can. Maj.	6 46	6 38 26	−13 41·3
	57 27 E		Sirius.....	2 50		

May 5. Assumed position of β Can. Maj., $6^h\ 16^m\ 22^s$, 17° 53′ 16″ S.
May 6 and 7. Comet growing faint; observations made with difficulty.

Capt. Shadwell has also communicated some physical observations of the same comet by M. Bosquet, the Government Meteorological Observer at the Mauritius. An account of these will be given in the *Notice* for November.

Comet III., 1853.

This comet, which was discovered by M. Klinkerfues at Göttingen, on the 10th of June last, became distinctly visible to the naked eye towards the end of August, agreeably to the prediction of M. Brühns (*Ast. Nach.* No. 869). It has even been observed in full daylight with the telescope, both by Mr. Hartnup and M. Julius Schmidt.

The following, which may be considered as the most recent

* Accompanying these observations was also a very neatly-executed chart, exhibiting the comet's path through the stars in its vicinity.

determination of the orbit of this comet is by M. D'Arrest (*Ast. Nach.* No. 875, p. 192):—

T ...	1853, Sept. 1·74969	M.T. Berlin.
ϖ	310° 58′ 29″·8	Mean Equinox, 1853·0.
Ω	140 31 3·4	
i	61 30 46·2	
Log q	9·4867092	

Motion direct.

Discovery of a New Comet. By M. Brühns.

On the night of the 11th September, M. Brühns, of the Berlin Observatory, discovered in the constellation of the *Lynx* a pretty large nebulous object, having some resemblance to a cluster of stars, but which from its proper motion he soon recognised to be a comet.

1853.	M.T. Berlin. h m s	R.A. ° ′ ″	Decl. ° ′ ″
Sept. 11	13 12 15·3	126 59 11·5	+44 51 33·6
13	12 6 31·0	129 33 3·7	+43 47 17·4
15	12 13 53·8	132 22 8·9	+42 29 54·5

The following elements of this comet have been calculated by M. Brühns from the observations of Sept. 11, 16, and 21:—

Comet IV. 1853.

T	Oct. 16·6304	
ϖ	301° 55′ 8″·8	Mean Equinox, 1853·0.
Ω	220 19 11·8	
i	61 15 55·2	
Log q	9·23106	

Motion retrograde.

Explanation of some Points relative to the Photometry of Telescopic Stars. By the Rev. W. R. Dawes.

In an Appendix to the twelfth volume of the Radcliffe Observations, recently published, is contained a series of highly interesting and valuable remarks and observations on the photometry of stars by means of the heliometer. In referring to a paper of mine, "On a Photometrical Method of Determining the Magnitudes of Telescopic Stars," which was printed in the *Monthly Notices* for June 1851, Mr. Johnson has suggested (see p. 16 of the Appendix), that in my observations, according to that method, I probably diminished not only the aperture of the telescope, but "also the pupil of my own eye to $\frac{3}{5}$ of its natural dimensions, and thus curtailed its capacity for receiving light:" and proceeding upon this

hypothesis, he shows, very ingeniously, that the discordance between his results and mine may be almost wholly removed. It may, therefore, be desirable that I should state, that my method has been in this respect misapprehended, and that *no diminution of the pupil* was employed in my telescopic determinations of magnitudes. The discordance of our results remains, therefore, to be accounted for.

It has also been supposed (see p. 17 of the Appendix), that the apertures employed by me in the determination of magnitudes, by reducing the light of the stars to a minimum, are not given in my paper above alluded to; and that they must have been different from those contained in the table on p. 191 of the *Monthly Notices*. The apertures there stated, however, are those which I actually employed, as being *suitable to my own eye*, which requires an aperture of $0^{in}.15$ to render visible Argelander's average sixth magnitude. The minimum visibility of a star with any one of the apertures in that table *determines* it to be of the magnitude corresponding to that aperture, and standing against it in col. 1. The heading of that column ("Σ's magnitudes") does not indicate that the numerical magnitudes composing it were *actually assigned by Struve to any particular stars observed and estimated by him*, but only that those magnitudes are arranged according to the ratio of progression employed by Struve and adopted by myself; as those in col. 2 represent the *theoretical* scale adopted by Sir John Herschel. In fact, the magnitudes in col. 1 are carried down to the 15th, and in col. 2 to the 24th; whereas Struve never estimated magnitudes below the 12th, nor Herschel below the 20th.

Remarks on the Application of the Heliometer to the Photometry of the Stars. By Manuel J. Johnson, Esq., Director of the Radcliffe Observatory, Oxford. (Appendix I. to vol. xii. of the Radcliffe Observations).

Before proceeding to give an account of his researches on this subject, Mr. Johnson explains a mode of estimating the magnitudes of the stars, which was practised by himself and his assistant Mr. Lucas, while engaged in reobserving the objects of Groombridge's Catalogue with the meridian instruments. This was founded on the degree of illumination which was necessary for the observation of the star, if under the sixth magnitude, and in the case of larger stars, on the visibility of the *corona* or halo of light by which the object was observed to be surrounded. In this manner he was enabled to form an unbroken scale of brightness, extending from stars of the fourth down to those of the tenth magnitude, the latter being the smallest stars visible in the transit telescope when the field of view was slightly illuminated. A very satisfactory accordance was found by Mr. Johnson to subsist between his own results and those of his assistant; and after some experience of the method he felt convinced that a tolerably accurate judgment of the relative brightness of the stars might be formed by employing it.

Subsequently, the observations with the heliometer suggested to Mr. Johnson the possibility of obtaining a photometric definition of his scale. In measuring the distance between two stars of unequal magnitude, it becomes necessary to reduce the aperture of one of the segments of the divided object-glass. It therefore occurred to him, that by reducing the aperture of the segment through which the brighter star is seen, until both stars are made to appear of equal magnitude, the ratio of light would be inversely as that of the squares of the diameters of the apertures. He afterwards found that this method had been recommended by Bouguer in the middle of the last century, and that it had been subsequently applied by several observers, including among others the Rev. W. R. Dawes, with whose researches he became acquainted some time after he had been engaged in the inquiry.

The plan pursued was very simple. A number of semicircles of card-board or stiff paper were prepared, having the same exterior diameter as the object-glass, with concentric apertures diminishing by $\frac{1}{4}$ of an inch from seven inches to half an inch. These fitting into the frame of the object-glass, were kept in place by knee-springs.

The objects under comparison having been brought into the field, the segment through which the bright star was seen, was reduced until its light was equalised to that of the smaller one. In the course of observations of this kind, Mr. Johnson found it necessary to institute a series of delicate experiments with the view of determining whether the two half object-glasses possessed equal facilities of transmitting light, and whether this was true for every part of the same segment.

It remained to compare the photometric results obtained in this manner with the magnitudes estimated with the meridian instruments, and with the corresponding estimates of other observers. This comparison is exhibited in a series of tables which Mr. Johnson has appended as the results of his researches.

Table I. contains observations to determine the scale of magnitudes used in the Radcliffe observations. The stars compared together range from the fourth to the ninth magnitude, but generally there is a preponderance of small stars. The mean ratio of the diminution of light, corresponding to an interval of a whole magnitude, was found by him to be 0·424. The mean error of a single comparison was 0·057, and as there were sixty of such comparisons, the mean error of the final result was 0·007.

Table II. contains the results of some experiments to test the accuracy of the ratio 0·424, when applied to stars of a higher class than those from which the ratio was mainly deduced: it also exhibits a comparison between the photometric magnitudes due to this ratio, and the estimated magnitudes of other observers.

From certain experiments made by Sir John Herschel on the relative brightness of the larger stars, Mr. Johnson found that the ratio of the light of a star of the first magnitude to one of the sixth magnitude is as 112 : 1. This indicates a diminishing ratio of

0·407, a result which agrees very nearly with the ratio which Mr. Johnson had deduced from his own estimated magnitudes.

Mr. Johnson also found that for stars from the first to the sixth magnitude, the numbers assigned by the photometric ratio 0·424 his presented a striking agreement with those given by M. Struve in *Catalogus Novus*, p. xxxv., and *Mensur. Microm.* p. lxviii., which was deduced entirely from considerations founded on the relative number of stars of each class, and on their equal distribution throughout space.

"Argelander and Groombridge," says Mr. Johnson, "apparently guided by similar considerations, both arrived at nearly the same *ideal* standards as ourselves, as the following comparisons will show. Our object-glasses, it will be remembered, are four inches in diameter, and we note to (10); Argelander's is 4·25 inches, and he noted also to (10); Groombridge's object-glass was 3·5 inches, and he did not go lower than the (9) magnitude.

Radcliffe Obs.	Groombridge.	No. of Comps.	Mean Error of One Comp.	Argelander.	No. of Comps.	Mean Error of One Comp.
3·0	2·8	11	±0·30	2·6	11	±0·45
4·0	3·9	20	·29	3·9	20	·32
5·0	5·0	20	·35	5·1	20	·25
6·0	6·1	20	·32	6·1	20	·30
7·0	7·0	40	·38	7·2	63	·38
8·0	7·9	40	·41	8·2	54	·39
9·0	8·9	40	±0·40	9·2	40	±0·37

From twelve photometric comparisons, Mr. Johnson deduced the ratio of diminution of light corresponding to a whole magnitude in Groombridge's scale to be 0·388, with a mean error of ±0·029; and from twenty such comparisons he found the ratio of diminution in Argelander's scale to be 0·431, with a mean error of ±0·015.

It is a remarkable fact that Mr. Dawes, by adopting a ratio of diminution equal to 0·25, obtained a series of magnitudes which appeared to him to present a satisfactory agreement with the estimated magnitudes of Argelander, although the latter have been found to correspond very nearly with those of Mr. Johnson, which proceeds according to a different ratio from that assumed by Mr. Dawes. Mr. Johnson has suggested an explanation of this discordance, in which, however, it will be seen that Mr. Dawes does not concur.

Table III. contains some observations made for the purpose of determining Struve's scale in his *Mensuræ Micrometricæ*. From a series of comparisons between the estimated magnitudes of Struve and his own photometric results, Mr. Johnson deduced the mean ratio of diminution of light to be 0·348, the mean error of a single comparison being ±0·036, and that of the entire result ±0·007. This result, however, he found to be true only for stars above the 6·7 magnitude. For smaller stars, M. Struve appears to have adopted a ratio which Mr. Johnson found on the whole to present a satisfactory agreement with his own, viz. 0·424.

It would seem also that Sir John Herschel, in his observation of double stars, recorded in the *Memoirs* of the Astronomical Society, adopted a different scale for stars above and below the sixth magnitude. Assuming the ratio of diminution to be 0·480 for the former class of stars, and 0·357 for the latter, Mr. Johnson showed that the estimated magnitudes would be represented with tolerable accuracy. It is worthy of remark that in this case the transition is from a higher to a lower ratio, whereas the contrary appears to hold good in the case of Struve's observations.

"As a concluding remark to this part of my subject," says Mr. Johnson, "I will recall the reader's attention to the fact that, in the course of the discussion, the law of diminution has been deduced from *five* different authorities, and another result from Otto Struve is given at p. 45. All these observers acted quite independently of each other, and though their instruments varied from the $3\frac{1}{2}$ to the 15-inch refractor and 18-inch reflector, yet their determinations, notwithstanding many and great individual anomalies, present a general appearance of consistency and agreement which can hardly be accidental.

"The several results are as follows:—Sir John Herschel's being that which was deduced from the light of *Sirius*; Struve's, his mean ratio from (1) to (12), which is his own limit; if we had taken (13), his value would become ·415, and the agreement would be more perfect.

Herschel	0·407	·Weight 1
Struve	0·383	2
Otto Struve	0·406	1
Argelander	0·431	3
Groombridge	0·388	1·5
Radcliffe Observatory	0·424	4

"Allowing the annexed weights to the respective results, the mean is 0·412, with a mean error of ±0·0065; and I have reason to believe that with a very slight modification this law will be found to hold good from *Arcturus*, taking Sir John Herschel's estimate of that star on his vulgar scale, through the intermediate stages to the (9) magnitude on Argelander's or our own."

Table IV. contains observations for the purpose of determining the variation of light of *δ Cephei*.

Mr. Johnson's observations of this star were made in the months of August and September 1851. They indicate the period of variation to be $5^d\ 6^h\ 36^m\ 20^s{\cdot}4$. By combining his own observations with Argelander's minimum of 1840, Nov. 1·503, he found the period to be $5^d\ 6^h\ 42^m\ 18^s{\cdot}4$.

Mr. Johnson concludes the account of his labours by pointing out the class of objects to which the heliometer is more especially adapted as an instrument of photometrical research. "The costliness of the instrument," he remarks, "will probably suggest an argument against its use. But this need not be an impediment. A half

object-glass of 4 inches will penetrate to a (10), which is only one magnitude higher than half of 7·5 inches will reach; and the instrument itself would be very much more convenient. A very ordinary equatoreal mounting would suffice, and no great expense need be incurred in its other fittings. Indeed, I am not sure if it would not be better to return to Bouguer's original construction, and employ two entire object-glasses of smaller dimensions. This, I imagine, would be a still more economical arrangement."

"It is a subject which may be taken up at any time: it does not require continuous exertion; it involves no tedious calculations. For these reasons it is well suited to amateurs, to whom, if I mistake not, a rich and interesting field is open, which professional observers, from the extent of their avocations, will be unable to enter."

Report addressed by the Astronomer Royal for Scotland to the Board of Visitors, at their Visitation of the Royal Observatory, Edinburgh, November 1852.

It will be seen from this report that Professor Piazzi Smyth has finally completed the reduction and printing of his predecessor's observations, and that he has already made considerable progress in preparing for the press his own observations of the years 1849–50–51. The class of objects, which has recently occupied his attention, has been already alluded to in the last Annual Report of the Society.

Steps have been taken for forming a galvanic connexion between the Observatory and the Railway Station at the Waverley Bridge. As soon as this shall be effected, the Observatory will be enabled to maintain an unbroken galvanic communication with the Royal Observatory of Greenwich, a circumstance which cannot fail to be attended with important advantages.

Professor Smyth remarks that the bright twilight which prevails during the summer months in high northern latitudes, and the generally cloudy state of the atmosphere, have proved very unfavourable to accurate observation, during the period when his duties in connexion with the University allow him most leisure for the prosecution of his labours at the Observatory. He proposes, therefore, that an arrangement should be made, enabling the Director to proceed annually with an equatoreal to some elevated region in a southern latitude, where he might execute a series of measures at a *temporary* observing station during the summer months; which measures in the autumn would be brought home, computed, and printed with all the resources of a civilised country, the results forming part of the usual Observatory Volume. He points out the peculiar advantages which the Peak of Teneriffe affords for this purpose. "As to the instrument itself," he adds, "I have devised a new construction of the equatoreal stand, which will allow of its being taken to pieces, and transported with great facility."

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CONTAINING

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ABSTRACTS OF PAPERS,

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REPORTS OF THE PROCEEDINGS

OF

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FROM NOVEMBER 1853, *TO JUNE* 1854.

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G. B. AIRY, Esq., President, in the Chair.

Robert Brewin, Jun., Esq., Birstall Hall, Leicestershire;
Benj. Dennison Naylor, Esq., Altrincham, Cheshire;
were balloted for and duly elected Fellows of the Society.

Extract of a Letter from Professor Hansen to the Astronomer Royal, dated 1853, *Nov.* 4.

"The work on the Lunar Tables has been greatly advanced during this last year. In the course of a fortnight we shall arrive at the definitive corrections of the elliptic elements for the present epoch, as well as the coefficients of the disturbances which depend on the sun's parallax and on the ellipticity of the earth; and I count upon an excellent agreement between the Observations and the Tables. For greater security these corrections are separately based on the Greenwich and the Dorpat observations; and, as far as I can foresee at present, a very good agreement will in this respect also be presented. From the discussion of these observations, however, it results that something yet remains depending on the difference of longitude between Greenwich and Dorpat. I have to-day written to Struve on this subject. The work which yet remains is the settling of the Epoch of the Moon's motion for remote times for the definitive determination of the mean motion. I hope, in the space of a year, to be quite ready with the Lunar Tables.

"The Solar Tables are at last printed."

Observations of Comet II., 1853, *made at the Royal Observatory, Cape of Good Hope.* By Thomas Maclear, Esq.

When the comet was detected shortly after nightfall on the 1st of May, the tail appeared to be about $3\frac{1}{2}$ or 4 degrees in length, and singularly curved or convex towards the south, near the head, conveying to the imagination the impression of a parabola, of which

the curve on the south side of the axis only was visible. The outline of the parabolic shaped head was sharply defined, and it seemed to be immersed in a very delicate coma or halo, which subsequently increased in dimensions.

In the direction of the axes there was no cometary matter, nor within the curve anywhere up to the nucleus; and the edge of the tail adjacent to the axis appeared to be sharper than the opposite edge.

In brilliancy, the nucleus might be safely taken equal to that of a star of the fifth magnitude; but, under high powers, the pungency of the light vanished, and the image spread fan-shaped in the direction of the head. It remained pretty bright until the 9th of May.

With respect to the observations, they were made by myself with the $8\frac{1}{2}$-feet equatoreal and spider-line micrometer, armed with the power 123; and, in general, the telescope was moved by the "driving clock," when the star and comet were simultaneously visible in the field.

It may be proper here to state, that a single micrometer measure for difference in right ascension seems to be at least six times more accurate than the difference by time transits over a single wire, where the objects are pretty well defined; and perhaps twenty times more accurate when one of them is a nebulous blotch, whose centre has to be estimated. There is a strong instance of this on the 7th of June.

For several days before its final disappearance, the comet resembled an oval-shaped nebula, slightly condensed in the middle. The last observation was on the 11th of June; the following three nights were clouded; afterwards it could not be detected.

The first and second columns give the date and Cape mean time of observation. The third column contains the observed differences in right ascension, corrected for refraction, between the comet and stars compared with it when derived from transits; or, when observed in terms of the micrometer-screw, the arc value reduced to time and multiplied by the sine of the N.P.D. of the middle point between the comet and the star. The fourth column gives the number of partial measures. The fifth gives the arc values, corrected for refraction, of the observed differences in N.P.D.; and the sixth gives the number of partial measures.

The signs + and —, prefixed to the differences in columns three and five, imply that the quantities are to be added to or subtracted from the R.A. and N.P.D. of the stars to obtain the R.A. and N.P.D. of the comet.

The seventh column gives the conventional numbers of the stars for reference. Their approximate places derived from the equatoreal circles, will be found on the last page sufficiently near for identification. Their correct positions will be observed at the proper season with the meridian instruments.

Royal Observatory, Cape of Good Hope,
Aug. 13, 1853.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 1	7 46 28·10	.....	...	+ 2 50·24	2	1	
	7 56 37·29	−1 46·532	2	.....	...	1	*t*
	8 6 17·36	.....	...	+ 3 38·17	2	1	
	8 26 37·32	.....	...	+ 4 25·86	5	1	
	8 37 17·28	0 34·479	1	.....	...	1	*t*
3	7 19 49·87	.....	...	+ 5 12·82	3	2	
	7 42 23·62	1 12·658	4	.....	...	2	*t*
	7 42 23·62	1 34·739	4	.....	...	3	*t*
	7 42 23·62	1 53·530	4	.....	...	4	*t*
	7 56 28·52	.....	...	+ 5 25·34	...	2	
	8 10 29·46	0 50·447	3	.....	...	2	*t*
	9 11 36·35	−0 1·623	8	.....	...	2	*t*
	9 18 32·71	.....	...	+ 5 41·45	...	2	
4	6 35 11·59	.....	...	− 4 56·02	2	5	
	6 57 18·62	+0 34·348	9	.....	...	5	*t*
	6 57 18·62	−0 17·970	9	.....	...	6	*t*
	7 14 42·60	.....	...	− 3 19·55	2	6	
	7 22 54·73	−0 3·370	9	.....	...	6	*t*
	7 35 59·60	.....	...	− 3 20·31	2	6	
	8 8 11·72	+0 25·340	5	.....	...	6	*t*
	8 30 21·54	.....	...	− 3 19·85	3	6	
	8 44 49·83	+0 44·486	3	.....	...	6	*t*
	8 10 53·45	−1 22·002	3	.....	...	7	*t*
	8 30 21·54	.....	...	− 1 4·42	3	7	
	8 50 28·63	−0 58·841	6	.....	...	7	*t*
	9 13 5·16	.....	...	− 1 7·67	2	7	
5	7 0 27·44	.....	...	+11 28·42	1	8	
	7 37 17·49	−7 29·912	1	.....	...	8	*t*
7	7 3 43·40	+1 14·379	4	.....	...	9	*t*
	7 19 37·18	.....	...	−10 13·21	2	9	
	7 36 1·49	+1 22·408	3	.....	...	9	*t*
	8 3 37·29	.....	...	−10 23·67	3	9	
	9 3 57·54	.....	...	−10 31·27	2	9	
	9 11 16·84	+1 47·781	1	.....	...	9	*t*
	7 3 43·40	−0 46·079	4	.....	...	10	*t*

Clouded on the 2d.

May 5. Owing to clouds these observations only could be obtained.

7. Angle of position of comet's tail, 118° 40′.
No. 10 is in the comet's tail.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 7	7 25 34·21	.....	...	− 7 36·70	2	10	
	7 36 1·49	−0 37·838	3	.....	...	10	*t*
	8 35 1·80	.....	...	− 7 52·92	2	10	
	9 11 16·84	−0 12·661	1	.....	...	10	*t*
8	6 23 36·26	−0 34·350	4	.....	...	11	*t*
	6 44 36·46	.....	...	− 1 28·88	2	11	
	6 23 36·26	−1 1·502	4	.....	...	12	*t*
	6 50 2·67	.....	...	+ 3 59·82	2	12	
	6 23 36·26	−2 20·278	4	.....	...	13	*t*
	7 0 22·20	.....	...	+10 54·81	3	13	
9	7 2 3·47	+2 31·153	2	.....	...	13	*t*
	7 30 53·78	+2 36·305	1	.....	...	13	*t*
	7 43 20·78	.....	...	+ 6 47·17	3	13	
	8 2 31·62	+2 42·029	5	.....	...	13	*t*
	8 19 33·00	.....	...	+ 6 39·63	2	13	
10	7 11 44·27	.....	...	− 3 6·40	2	14	
	7 30 39·76	−0 27·602	1	.....	...	14	*t*
11	6 36 25·27	+0 44·368	3	.....	...	15	*t*
	7 0 34·48	.....	...	+ 5 37·06	4	15	
	7 21 27·38	+0 50·129	4	.....	...	15	*t*
	8 12 59·09	.....	...	+ 5 20·65	4	15	
	8 32 5·06	+0 58·440	4	.....	...	15	*t*
	6 36 25·27	−1 37·933	3	.....	...	16	*t*
	7 0 34·48	.....	...	+ 1 2·07	4	16	
	7 21 27·38	−1 32·201	4	.....	...	16	*t*
	8 12 59·09	.....	...	+ 0 45·13	4	16	
	8 32 5·06	−1 23·953	4	.....	...	16	*t*
12	6 41 44·35	.....	...	+ 1 20·44	6	17	
	6 54 57·74	−0 1·429	8	.....	...	17	*d*
	7 2 55·05	−0 0·686	9	.....	...	17	*d*
	7 15 7·60	.....	...	+ 1 14·82	9	17	
	7 36 9·97	+0 2·506	14	.....	...	17	*d*
	8 10 38·13	.....	...	+ 1 5·85	5	17	
	8 19 36·27	+0 6·752	5	.....	...	17	*d*

May 9. Angle of position of comet's tail, 121° 30′. Nucleus remarkably bright.
10. Clouded after these observations.
11. Angle of position of comet's tail, 115° 40′. Nucleus less bright; halo round the head more diffused.
12. Angle of position of comet's tail, 117° 30′.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 14	7 27 56·82	.....	...	− 5 52·53	3	17	
	8 4 32·88	+4 28·769	5	.....	...	17	*t*
	8 42 50·40	.....	....	− 6 6·18	5	17	
15	6 32 58·31	.....	...	+ 3 34·95	5	18	
	6 49 34·79	−0 37·701	5	.....	...	18	*t*
	7 7 42·58	−0 34·606	5	.....	...	18	*d*
	7 24 52·76	.....	....	+ 3 27·48	5	18	
	7 40 2·46	−0 32·717	5	.....	...	18	*d*
	7 49 22·93	.....	....	+ 3 22·71	5	18	
	8 0 34·68	−0 31·393	5	.....	...	18	*d*
	8 11 40·64	.....	....	+ 3 20·37	5	18	
16	6 20 43·76	.....	...	+ 7 42·41	5	19	
	6 32 35·81	−0 26·035	5	.....	...	19	*d*
	6 40 49·26	.....	...	+ 7 38·85	5	19	
	6 53 34·76	−0 24·743	5	.....	...	19	*d*
	7 3 28·35	.....	.....	+ 7 37·75	5	19	
	7 14 24·55	−0 23·516	5	.....	...	19	*d*
	7 21 15·63	.....	...	+ 7 33·79	5	19	
	7 37 17·40	−0 21·992	5	.....	...	19	*d*
	7 49 10·05	.....	....	+ 7 28·26	5	19	
	7 57 44·84	−0 20·916	5	.....	...	19	*d*
	8 21 20·18	.....	....	+ 7 23·12	5	19	
17	6 15 6·14	−0 16·685	5	.....	...	20	*d*
	6 16 0·00	−0 21·075	5	.....	...	21	*d*
	6 58 51·77	.....	...	− 7 56·53	5	20	
	7 5 4·55	.....	...	+ 3 26·32	5	21	
	7 16 43·34	−0 13·617	5	.....	...	20	*d*
	7 18 12·00	−0 18·014	5	.....	...	21	*d*
	7 28 54·65	.....	...	− 8 0·03	5	20	
	7 34 51·87	.....	...	+ 3 26·51	5	21	
	7 47 36·38	−0 12·086	5	.....	...	20	*d*
	7 48 38·21	−0 16·387	5	.....	...	21	*d*
	8 4 19·63	.....	...	− 8 0·31	...	20	
	8 9 58·11	.....	...	+ 3 17·34	5	21	
20	6 42 20·46	+0 12·428	5	.....	...	22	*d*

May 14. Angle of position of comet's tail, 118°. Clouds.
15. Angle of position of comet's tail, 117° 35′.
17. Angle of position of comet's tail, 118° 45′.
20. The comet is faint, partly owing to moonlight. The tail barely distinguishable.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
May 20	6 49 23·50	.....	...	− 0 48·61	5	22	
	6 55 1·77	+0 11·957	5	.....	...	22	*d*
	7 2 55·48	.....	...	− 0 51·63	5	22	
	7 8 59·68	+0 11·303	5	.....	...	22	*d*
	7 15 43·57	.....	...	− 0 50·25	5	22	
	7 21 17·66	+0 10·887	5	.....	...	22	*d*
21	6 41 26·83	.....	...	+ 6 3·15	5	23	
	6 49 47·26	−0 14·373	5	.....	...	23	*d*
	6 55 44·28	.....	...	+ 6 3·05	5	23	
	7 2 29·57	−0 14·653	5	.....	...	23	*d*
	7 9 28·82	.....	...	+ 6 1·23	5	23	
23	6 35 26·93	.....	...	+ 7 51·42	5	24	
	6 48 36·57	−0 2·714	5	.....	...	24	*d*
	7 0 21·44	−0 2·586	5	.....	...	24	*d*
	7 9 41·51	−0 2·157	5	.....	...	24	*d*
	7 18 44·82	.....	...	+ 7 41·29	5	24	
28	6 27 55·21	.....	...	+ 4 55·45	5	25	
	6 45 59·48	−0 30·305	5	.....	...	25	*d*
	6 56 48·34	−0 30·334	5	.....	...	25	*d*
	7 6 1·61	.....	...	+ 4 51·97	5	25	
29	6 13 20·56	−0 6·578	5	.....	...	25	*d*
	6 20 29·69	.....	...	+ 2 32·30	5	25	
	6 29 17·02	−0 6·249	5	.....	...	25	*d*
	6 35 10·81	.....	...	+ 2 30·40	5	25	
	6 40 52·64	−0 5·963	5	.....	...	...	*d*
31	6 24 32·60	+0 37·004	5	.....	...	25	*d*
	6 31 0·12	.....	...	− 2 5·16	5	25	
	6 37 46·37	+0 36·985	5	.....	...	25	*d*
	6 46 57·97	.....	...	− 2 9·65	5	25	
	6 54 6·78	+0 36·935	5	.....	...	25	*d*
June 1	6 27 12·98	.....	...	+10 2·01	5	26	
	6 34 57·26	−0 47·934	5	.....	...	26	*d*
	6 41 52·68	.....	...	+ 9 58·94	5	26	
	6 50 17·87	−0 47·377	5	.....	...	26	*d*
	7 14 34·44	−0 48·083	3	.....	...	26	*d*

May 23. Thin clouds. The comet is very indistinct.
29. The comet is very faint.
June 1. The measurements diminish in precision, owing to the faintness of the comet.

1853.	Cape Mean Time.	Difference in R.A.	No. of Obs. in R.A.	Difference in N.P.D.	No. of Obs. in N.P.D.	No. of Star.	
	h m s	m s		′ ″			
June 1	7 14 34·44	+1 0·237	3	.....	...	25	*t*
	7 24 49·65	.....	...	− 4 23·62	5	25	
2	6 27 25·54	+1 18·965	3	.....	...	25	*t*
	6 45 16·64	.....	...	−6 26·94	4	25	
	7 1 22·02	+1 19·253	2	.....	...	25	*t*
	6 27 25·54	−1 24·261	3	.....	...	27	*t*
	6 45 16·64	.....	...	−2 53·07	4	27	
	7 1 22·02	−1 23·896	2	.....	...	27	*t*
3	6 36 53·39	.....	...	+5 54·40	5	26	
	6 45 8·39	−0 11·407	5	.....	...	26	*d*
	6 52 20·58	.....	...	+5 53·80	5	26	
4	7 7 22·67	.....	...	+3 53·75	1	26	
	7 13 12·69	−0 5·529	2	.....	...	26	*d*
	7 25 19·64	.....	...	+3 47·42	2	26	
5	6 34 48·27	.....	...	+2 4·69	3	26	
	6 45 21·34	+0 21·519	5	.....	...	26	*d*
	6 55 12·79	.....	...	+2 3·91	3	26	
7	6 35 58·61	.....	...	−1 22·35	3	26	
	6 47 3·93	+0 51·866	10	.....	...	26	*d*
	6 58 43·86	.....	...	−1 24·26	5	26	
	7 16 36·13	+0 54·550	10	.....	...	26	*t*
	7 31 40·34	.....	...	−1 26·34	2	26	
8	6 49 9·52	+1 8·199	5	.....	...	26	*t*
	7 7 54·37	.....	...	−2 59·49	10	26	
	7 26 44·26	+1 9·505	3	.....	...	26	*t*
9	6 23 37·43	.....	...	−4 22·13	5	26	
	6 38 11·42	+1 23·619	7	.....	...	...	*t*
	6 56 13·48	.....	...	−4 23·55	5	26	
10	6 23 48·76	.....	...	−5 47·12	5	26	
	6 39 39·45	+1 38·681	5	.....	...	26	*t*
	6 54 45·86	.....	...	−5 48·28	5	26	
11	6 34 14·35	.....	...	+4 51·74	5	28	
	6 45 47·37	−0 22·350	5	.....	...	28	*d*
	6 55 0·64	.....	...	+4 50·27	5	28	

June 3. The comet is very faint.
4. Generally clouded. These observations only could be obtained.
8. Hazy atmosphere. The measures very difficult.
10. The moonlight is now diminishing the visibility of the comet.

Approximate Positions of the Stars compared with the Comet.

Reference Number.	R.A.	N.P.D.	Mag.	
	h m s	° ′ ″		
1	5 0 2	103 19 23	7	Preceded by a small star
2	5 57 29	104 4 54	7	H. C. No. 11551
3	5 57 51	104 1 37	8½	
4	5 58 10	104 6 51	8½	
5	6 11 52	104 18 18	10	
6	6 12 44	104 16 42	9	
7	6 14 32	104 14 30	8	
8	6 32 32	104 1 0	5	H. C. No. 12825
9	6 40 7	104 16 15	6	B. A. C. 2221
10	6 42 8	104 13 38	9?	
11	6 47 36	104 3 47	9½	
12	6 48 2	103 58 21	8	H. C. No. 13392
13	6 49 21	103 51 30	5½	B.A.C. No.2273: μ Canis Majoris
14	6 56 20	103 57 20	9	
15	6 58 15	103 45 0	8½	
16	7 0 37	103 49 30	8	H. C. No. 13848
17	7 1 49	103 45 28	9	
18	7 8 37	103 32 48	8	
19	7 9 59	103 25 24	7?	
20	7 11 10	103 37 49	7½	H. C. No. 14228
21	7 11 14	103 30 18	8	
22	7 14 22	103 21 24	10	
23	7 14 46	103 11 35	10	
24	7 16 34	103 4 9	10	
25	7 19 45	102 53 48	7½	
26	7 21 35	102 39 30	9	Preceded by a faint star, nearly on the same parallel.
27	7 22 28	102 50 20	10	
28	7 23 50	102 28 50	10½	

Extract of a Letter from A. G. Constable, Esq. to the Astronomer Royal, dated Lima, June 1853.

"I extract from my journal, premising that the bearings, &c., I received from the captain of the ship (in which I was only a passenger from Melbourne to Callao), are correct.

"May 1, 1853. This evening, at 7 minutes to 7 p.m., observed a comet at an altitude of 20° 47′ 30″, bearing W. by S. ½ S., dis-

tant from the lowest star in *Orion's* belt (*Bellatrix*), 16° 29′ 30″, and bearing from that star about S.S.W.

"At noon of that day our latitude was 34° 39′ 8″, and longitude 167° 54′ 4″, and since then we have been steering towards the N.W. end of New Zealand with a six-knot breeze.

"The comet appeared to be a star of the first magnitude, rather dim, and of a reddish hue, with a bright tail of 4° in length.

"P.S. Although anxiously watched for, the comet did not again appear to us."

The following communication (referred to in the Supplement to the June *Notice*) has been received from Capt. Shadwell, containing an account of physical observations of Comet II., 1853, made at the Mauritius by M. Bosquet, the Government Meteorological Observer at Port Louis.

"The Observatory of which M. Bosquet has charge is one of a purely meteorological character, and is unfurnished with any astronomical instruments, so that on the appearance of the comet that gentleman was only able to record the general physical appearance exhibited by that body.

"April 28. Soon after sunset there was a remarkable appearance of the zodiacal light, rising in a conical form to an altitude of about 50°, and extending into the constellation of *Orion*. This phenomenon lasted $6^h\ 45^m$ p.m.

"On the 29th and 30th, the evenings were more or less obscured by clouds.

"On May 1st, M. Bosquet distinctly observed the comet, and thus describes its appearance. The nucleus appeared with the brilliancy of a fourth-sized star, and the tail, nearly 10° long, was projected from the sun towards the constellation of *Orion*. The nucleus was surrounded by a vapoury appearance.

"On May 2d the tail of the comet seemed to have considerably diminished in length and brilliancy. The nucleus appeared smaller, and no bright point could be observed in the middle with the naked eye, as on the night preceding. The nucleus was nearly in a straight line between ι *Orionis* and α *Leporis*.

"May 3. During the evening the weather was very clear, and the whole body and tail of the comet could be seen of a faint and pale colour. With the naked eye the nucleus seemed of a very pale light, and no bright points could be discovered within it; but viewed through a telescope, with a power of 60, a node or body was distinctly visible in the nucleus. The tail projecting towards θ *Canis Majoris*, extended over it as well as over some other stars of the 4th and 5th magnitude, which could be distinctly seen through it.

"May 4th. The comet this evening had again diminished in size and brightness. To the naked eye it seemed as a faint light like a cluster of nebulæ, and had a very short tail. With a telescope the tail could be distinctly observed; its direction was slightly changed,

being a little more inclined towards the southward, and diverging more from the equator.

"May 5th. Comet gradually vanishing; stormy to-night; a faint whitish light. No observations taken subsequently to the 5th. The comet remained faintly visible till the 8th.

"*Note.* The estimated measurements of the length of the tail, as recorded both by Mr. Wright and M. Bosquet, were eye estimations, and not in any case obtained instrumentally.

"CHARLES F. A. SHADWELL, Capt. R.N.

"P.S. Finding, on comparing notes with Mr. Maclear at the Cape of Good Hope, that the comet had not been observed at the Cape before the 1st of May, and, therefore, that the observation of April 30th on board the Lady Jocelyn would be valuable, I have recomputed the comet's place, correcting it for refraction, and find as follows:—

App. Gr. Mean Time, $4^h\ 17^m$, April 30th,
R.A. $3^h\ 57^m\ 36^s{\cdot}3$ Decl. $11°\ 26'{\cdot}4$

using Dr. Inman's Refraction Tables, which are based on Dr. Young's formulæ."* C. S.

Extract of a Letter from Lieut. John Parish, commanding H.M.S. Sharpshooter at Rio de Janeiro, May 14, 1853.

"We had a very good sight of a comet whilst out on our last cruise, on the night of the 30th of April, bearing W. $\frac{1}{2}$ S. from our anchorage in Anuaçoa Bay, about 20 miles north of Rio Janeiro. Altitude 9° 2′, distance from *Sirius*, 35° 32′, from *Bellatrix*, 23° 51′, length of tail from 8° to 10°. Visible also on the night of the 1st of May, in nearly the same position. On our return I found it had been seen at Rio de Janeiro on the night of the 30th April.

"Has it been seen in Europe? If not this account of it may be interesting to astronomers.

"Had the weather been fine, I could have given you more particulars. It was first seen about $7^h\ 20^m$ p.m., and set at about 8 p.m."

Extract of a Letter from Capt. King, of Paramatta, N.S.W.

"April 30. A comet suddenly made its appearance after sunset. Angles from it to *Sirius*, 44° 19′, *Rigel*, 22° 35′, *Bellatrix*, 30° 20′.

"May 1. Clouded.

"May 2. Passing between λ and ν *Leporis*, about 18° 35′ from *Sirius*, towards which it was travelling. R.A. about $5^h\ 14^m$, decl. 13° south. Comparing this position with its place on the 30th

* Capt. Shadwell had not at this time learned that the comet had been previously discovered in Europe.

April, it had changed in R.A. 25°, and in declination 2° to the south, which will give for its daily motion in R.A. 75′·0, and in declination 60′. There was no star sufficiently near it to ascertain its position with a ring micrometer.

"May 3. Sextant angle between the comet and the following stars. *Sirius*, 12° 5′, *β Canis Majoris*, 7° 30′ (?), *Canopus*, 38° 58′, *Procyon*, 32° 16′, *Betelgrave*, 21° 41′, and with a ring micrometer at $7^h\ 58^m\ 28^s$ mean time (measured from *η Leporis*), it was, in R.A. $5^h\ 58^m\ 42^s$, decl. 14° 8′ 50″ south.

"May 4. Very indistinctly seen with a night-glass, and not discernible through a telescope, is about R.A. $6^h\ 12^m$, decl. 15° 45′ south.

"When first seen on the 30th, it was a very beautiful object, the nucleus and coma of rather large dimensions, but much diffused in the hazy medium, for it was low in the western horizon. The coma was about 4° in length, and the nucleus apparently of one-fourth the dimensions of the full moon. On the 2d May its form was more defined, the coma more linear, and the nucleus had a stellar body of apparently half the size of *Jupiter*, and much resembling that planet as if seen through a mist. As the comet descended, this appearance vanished, and the nucleus resumed the form it had on the first night (30th). On the 3d the stellar nucleus was not visible. Otherwise, except in its reduced size, it was the same as seen on the 2d. On the 4th it was much more indistinct, and the succeeding nights were also too cloudy to see it.

"It is much to be regretted that this, now becoming very important part of the world, is not considered to be worthy of the re-establishment of the Observatory, particularly as there are now several good instruments rusting in cases in the Government stores.

"A time-ball establishment is talked of, and during the last Session of Council a sum of money was voted for its erection; but as yet nothing has been done towards its completion. It is purposed to adapt the observing apartment for two meridional instruments, probably a transit and a mural circle. Room is left for the addition of a dome-room, for an equatoreal or altazimuth; but nothing will be done unless there be some impulse from without."

Note on Comet III. 1853.

(*Extract of a Letter from Dr. Foster, of Brussels, dated October* 4, 1853.)

"On Sunday, the 28th of August, the atmosphere being clear of vapour, I got a very good observation, and was enabled to apply some high power to it, owing to the extreme transparency of the air. In every telescope it had the same *clear and well-defined globe appearance*, of a bright golden colour, which was very distinct from the coma."

Daylight Observations of Comet III. 1853.

By John Hartnup, Esq.

The rapid increase in the brightness of the comet towards the latter end of August led me to think it possible that it might be seen with the sun above the horizon, and fortunately the 3d of September turned out a favourable day for such observations. Cirrocumuli clouds were prevalent, but between those clouds there were patches of clear blue sky. At about a quarter before 1 p.m. I set the equatoreal to the position of the comet deduced from the ephemeris published in No. 869 of the *Astronomische Nachrichten;* and on applying my eye to the telescope, the comet was immediately detected near the centre of the field. Its appearance was planetary; the nucleus was round, well defined, of a whitish colour, and about 9″ diameter. I could not detect the slightest appearance of a tail. The power used was 92; but, in order to obtain the position of the comet more accurately, I put on the parallel wire micrometer (power 132), with which it was well seen: but before I could complete my arrangements for an observation, clouds came over, and I did not see it again till after 2 p.m. Between 2 and 4 p.m. I saw the comet again two or three times for very short intervals; but it was exceedingly faint, and clouds and haze prevented further observation.

The following observations were obtained in daylight sufficient for me to see the spider-lines in the micrometer without the aid of illumination:—

1853.	Green. M.T.	R.A.	Log $\frac{p}{P}$	N.P.D.	Log $\frac{q}{P}$
	h m s	h m s		° ′ ″	
Aug. 29	7 33 0·2	11 34 26·63	+8·617	74 12 15·27	−9·8930
	47 59·8	34 25·48	·613	13 37·33	·8987
30	7 29 17·6	11 32 38·28	+8·611	76 25 10·68	−9·8970

The observations are corrected for refraction.

The comparison star for all the observations was *β Leonis*, and the following is the assumed mean place for 1853·0, deduced from the Greenwich Observations, 1849–50:—

R.A.	N.P.D.
h m s	° ′ ″
11 41 33·53	74 36 22·51

On the 29th, the tail, which was directed from the sun, consisted of two curved rays of light, which united at about three degrees from the nucleus.

Discovery of a New Planet at Mr. Bishop's Observatory.

By J. R. Hind, Esq.

"I have to announce my discovery of another new planet on the 8th of the present month, at $7^h\ 50^m$ mean time, not very far from the place where I found *Thalia* in December last. It shines

as a star of the ninth magnitude, and evidently belongs to the group between *Mars* and *Jupiter*.

"By micrometrical comparisons with No. 6361 of Lalande's Catalogue, the following positions were deduced:—

M.T. Regent's Park.		App. R.A.	App. Decl.
	h m s	h m s	° ′ ″
Nov. 8 at	8 2 46·6	3 19 33·50	+16 4 32·3
—	8 34 1·2	32·17	28·4
—	11 21 58·4	24·93	11·4
9 —	9 2 31·1	3 18 30·93	+16 1 18·0

"The place of Lalande's star was taken from a Greenwich observation on November 9th, with which the Astronomer Royal kindly furnished me.

"*Mr. Bishop's Observatory, Nov.* 11."

This planet has received the name of *Euterpe*.

First Elements of Euterpe.

Computed from the Observations of Regent's Park, Nov. 8, Durham, Nov. 12 and Nov. 17, 1853, by Professor Chevallier and Mr. George Rümker.

Epoch, 1853, Nov. 20·0, Greenwich Mean Time.

	° ′ ″	
M	326 24 19·4	
π	100 42 2·0	Mean Equinox,
λ	91 3 45·6	Jan. 0, 1854.
i	1 45 47·9	
φ	10 23 47·8	
Log a	0·385910	
Log μ	2·971142	

These elements represent the middle observation with an error (computed—observed) of −0″·1 in Longitude, and 0″·0 in Latitude.

Professor Chevallier has also caused to be printed an Ephemeris of the planet based on the foregoing elements, and extending from Nov. 20 to Dec. 31. Any person may obtain a copy of this Ephemeris by applying at the apartments of the Society, to which a supply of copies has been liberally forwarded by Professor Chevallier for distribution.

Notice respecting his Observatory at Red Hill.

By R. C. Carrington. Esq.

"It has been known to several of the members of the Society for some time past, that I have been preparing to resume my former occupation as an observer in a different part of the country, and with the intention of following a different line of observation to that which formed my duty at Durham. Many months were necessarily

taken up in finding a site suitable for a meridian observatory, in building, in providing instruments, and in getting into order again. As I have now been at work for three or four months, and can produce some results of observation, I think the time has come for giving some notice to the Society of the class of work I am engaged in.

"To be as brief as possible, I have planned my observatory entirely with reference to zone-observation on the meridian, and the formation of accurate maps of portions of the heavens. Any other subjects taken up will be temporary or subservient to the above. One such subject will probably be the positions of spots on the sun, if not found to interfere prejudicially with our time for reduction.

"In the execution of this plan I propose to establish one or two distinctions between any catalogue I may live to produce and those which we have already in existence. In the first place, I intend to take such measures as shall effectually guard against the omission of any star included within what is now called the tenth magnitude; secondly, that the zones of observation shall be incorporated into catalogue-zones of 9° extent in N.P.D., reduced to mean places of an epoch common to the zone, in which every star, to the tenth magnitude inclusive, within the limits, will have its reference number; and thirdly, that each star shall be sufficiently often observed, or otherwise checked, that no errors of whole minutes or graduations shall remain to render its present existence or future identity doubtful. With respect to the maps, I propose that while the scale shall be sufficiently large to enable configurations to be satisfactorily represented, the size of the maps themselves shall not exceed what is found convenient to hold in the hand while comparing with the telescope, and further, that the places of all nebulæ mistakable for comets shall be plainly indicated. To the above it will be my endeavour to add the searching out and approximate measurement of all multiple stars that have hitherto escaped notice, and the determination of the magnitude of each star by some more accurate standard than the unaided judgment.

"It is, I conceive, only by an united system of cataloguing and mapping that a faithful history of the heavens of our times can be transmitted, by which alone those changes can be detected, which will hereafter form new subjects of inquiry.

"It may be only my private opinion, but I consider that with the excellent catalogues of Bessel and Argelander, now so accessible, any future author of a catalogue pretending to the character of a survey would be doing less than the progress of astronomy now demands, who should not bind himself to some such rules as the above.

"The portion of the heavens to which my attention is now, and will be for some time, entirely confined, is the region within 9° of the north pole. I estimate that, to observe, reduce, and map the stars therein contained, will occupy me from two to three years, taking into account the climate.

"My present instruments are a transit-circle and a small equatoreal, constructed by Mr. Simms, who undertook both the optical and mechanical part, and who has not been unmindful of his well-earned reputation. The transit-circle has 5 inches aperture, $5\frac{1}{2}$ feet focal length, 5 feet axis, two circles of 40 inches diameter, the graduated one being divided in gold on a bevelled face, and read by four microscopes, arranged in a cone, as at Greenwich. There is a counterpoise apparatus by which the residual pressure is reduced to about 15 lbs. on each bearing. There are two fixed collimators of 30 inches focal length. The axis and circles are of gun-metal, the telescope cones of hammered brass. The piers contain about 10 tons of Caen stone, resting on a thick slab of York flag-stone, laid on a considerable mass of concrete,—arrangements which I superintended personally throughout. The building is so planned that the transit-room is bounded on both sides by occupied rooms, in which the fire-places are removed to the opposite sides: the party walls are of brick, 12 inches thick: the north and south walls are of stone, 14 inches thick, and *without windows*. The level error was 0″·7 on Aug. 1st, at starting, and 1″·8 on Nov. 1st, the change having been very gradual and pretty constantly watched: the meridian error has varied a very little more in amount. The only objection to the abolishment of windows that I have traced is, that it appears to favour the generation of spiders. The equatoreal has $4\frac{1}{2}$ inches aperture and $4^{ft.}\ 4^{in.}$ focal length. I usually employ a power of 28 and a field of a degree and a half: it is suited for rough zoning, sweeping, mapping, and screen-projection. It is German-mounted on a cast-iron stand of my own design, which I think possesses some advantages. The clock I now have was made by the late H. Appleton, of 30 Southampton Row, Russell Square: it has the dead-beat escapement; its rate is excellent, but the beat dull, and I have still some difficulty in this respect to get over before I can set at defiance the sounds of a busy neighbourhood.

"At the next meeting of the Society I hope to lay on the table printed copies of nine maps, containing the results (above alluded to) of a first preliminary exploration of the region I have named, and which was brought to a close within the present week. The scale is 1·5 inches to the degree, and 4268 stars have been laid down, the mapping having been purposely carried beyond the proposed limit for reasons which I will then explain.

"The approximate position of the observatory is

$51^{\circ}\ 14'\ 25''{\cdot}25$ North
$0^{m}\ 41^{s}{\cdot}25$ West of Greenwich,

which is sufficiently accurate for my present purpose, but open to amendment.

"Mr. G. H. Simmonds, who was with me for some months at Durham, is my assistant at Red Hill.

"The post-town for Red Hill letters is Reigate, Surrey."

On the Zodiacal Light. By E. J. Lowe, Esq. F.R.A.S. F.G.S.

During the past spring the observations on the *western light* have been but few, owing to the great prevalence of cloud during the early portion of the evening. However, as on several occasions some of the features were well marked, I have forwarded these records as a continuation of my former reports on this subject.

Epoch 1853, January 31st, $6^h\,45^m$.

The light was very brilliant for half an hour, the north edge being most clearly defined. The north edge passed between No. 77 and No. 82 *Pegasi*, and the south edge near *i Ceti*. The apex of the cone advanced and receded in half-minute intervals as much as 5°, its greatest altitude being *ε Piscium:* owing to a stratus stretching along the valley its extent on the horizon could not be well observed; by producing the two edges it was thought to cut the horizon 10° N. of W. and 30° S. of W.

Epoch 1853, February 27th.

Faint and confused; aurora borealis occurring at the time.

Epoch 1853, March 8th, $7^h\,45^m$.

Somewhat brilliant, yet only occasionally well defined on the edges. It had pulsations of greater and less brilliancy, and at times the north edge was clearly defined; at such periods the north edge passed about 10′ S. of *γ Arietis;* the light seldom reached this star, being mostly midway between *γ* and No. 2 *Arietis:* the star *η Ceti* was also near upon the edge. The cone rose to the altitude of *Saturn*, but was slightly north of that planet, the apex being midway between *Saturn* and the *Pleiades in direction*, but not higher than the altitude of *Saturn.* The base was hidden by a stratus. The stars were alternately dimmed and again bright beneath it; some were constantly disappearing, perhaps twice a minute, more especially in the centre of the phenomenon: once bars, or streams of light, were apparent on the north edge. At this moment the light was brightest, and the north edge most clearly defined.

Epoch 1853, March 29th, $8^h\,30^m$.

The light tolerably bright. The south edge passed midway between *ο Tauri* and *λ Arietis;* the north edge *did not keep a fixed position*, fluctuating from 2° beyond *α Arietis* to midway between *α* and *β Arietis;* the apex also oscillated from the *Pleiades* to 2° beyond that group (being an oscillation of 2° vertically). The extent on the horizon was 30°. The axis passed through *π Arietis.* The light was brightest along the axis. Stars were slightly dimmed; they also did not scintillate so much *within* as *without* the phenomenon. At one period ($8^h\,45^m$) there was a double edge on the north side, the inner (or new edge) being very brilliant, and passed 4° W. of *α Arietis.* This second edge was visible 5^m. The pulsa-

tions of brightness were in about half-minute intervals, both horizontally and vertically at the same time. The phenomenon was most brilliant when most expanded.

I feel persuaded that the periodical change in brightness and the expansion and contraction of the cone is not produced by our atmospheric influence, as it is always apparent when the phenomenon is brilliant, whatever be the hygrometrical state of the air. It appears to me to be owing to *a rotation of the cone*, and that the light is not always *equally bright* in all parts. In those countries where the zodiacal light is very brilliant it would be well to observe these features carefully.

On the Telescopic Appearance of Saturn. By the Rev. W. R. Dawes.

"The present autumn has hitherto afforded so few opportunities for delicate observations with high powers, that the following results of two or three examinations of *Saturn*, under favourable circumstances, may not, it is hoped, be unacceptable to the Society.

"1853, Oct. 24, $12^h\ 45^m$ G.M.T. $8\frac{1}{2}$-foot refractor, powers 350, 460, and 640. When best seen, the *dark line* on the outer bright ring (A) was so plain that it could scarcely be overlooked during careful scrutiny of the ring. Occasionally, when particularly fine views occurred, the line seemed to extend itself a long way round; but usually not further than a tangent to the inner edge of the inner bright ring (B). It seemed equally visible at both ends of the ring, and occupies precisely the same position as it did last year and the year before, viz. just outside the middle of the ring.

"The nebulous ring (C) was extremely obvious, and struck me as rather *brown*. At times there seemed to be an exceedingly narrow dark line between the rings B and C at the ansæ, but I could not feel sure of it. The interior edge of the ring C was very well defined, and the diminution of its brightness towards its inner edge seemed to me unquestionable. The ball was very plainly seen through the nebulous ring; more plainly than I ever saw it before; the circumstances being more favourable than any of which I was able to avail myself last year.

"The *belts* are not so decided as they were last year, except the narrow belt round the south pole, which is very complete and uniform. It is of a neutral tint; but the southern hemisphere of the planet, south of the broad brownish band near the equator, certainly has a greenish hue. The broad band is not distinctly separated from the lighter shade south of it; nor is it decidedly divided into two by a bright line as it was last year, though its surface is obviously not uniform.

"While scrutinising the southern portion of the planet, I was struck with the appearance of a small black spot on the *eastern* side of the ball upon the ring B, just within the division of the rings A and B. When most sharply defined, it appeared somewhat triangular. It is, no doubt, the same as was noticed by Mr. Lassell

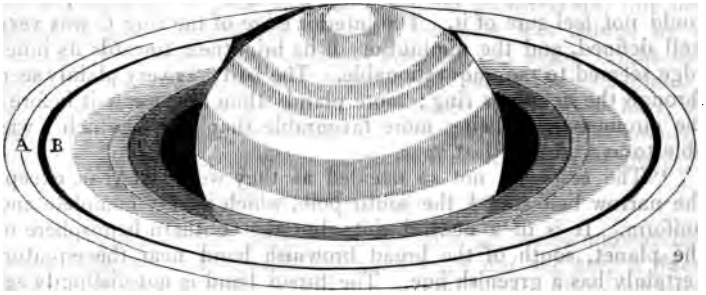
last year at Malta, and seems to be the projecting part of the shadow of the ball on the ring B. But I was amazed to see it so early, while the shadow and the *western* side is yet so broad. The edge of this shadow is most decidedly *convex* towards the ball on the ring B, and *concave* towards the ball on the ring A. There is an obvious *notch* on the edge of the shadow, which extends much further from the ball at the outer edge of B than at the inner edge of A, imparting the impression that these two edges are not in the same plane. It is far greater than is due merely to the *separation* of the two rings.

"Oct. 25, 11^h G.M.T. I entered in my journal as follows:—'Fine views, confirming all I saw last night. Vision is more steady and satisfactory. The black spot on the ring B is seen, I think, more plainly than last night. The southern edge of the ball seems to be exactly coincident with the southern (exterior) edge of the outer ring. The dark line on that ring at times very plain and steady. Nebulous ring very well seen; *western end browner than the other*. Quite transparent where it crosses the ball.'

"'Nov. 2. There is a thick haze over the sky, through which the brighter stars are shining dimly with constantly varying brightness. *Saturn* at times remarkably distinct. The black spot on ring B to the *east* of the ball is well seen: it has certainly increased since Oct. 24 and 25, when I first saw it.

"'The nebulous ring is wonderfully well seen. When the haze thickens, the fading (or shading off) of the light of this ring on the portion towards its inner edge is very obvious.'

"I inclose a small sketch of the planet, which, though it has no pretensions to great accuracy in the proportions, and shows the shadows somewhat exaggerated, may serve to illustrate the verbal description.



"*Wateringbury, 9th Nov.*, 1853."

On the Visibility of the Dark Ring of Saturn in a moderate Telescope. By T. W. Burr, Esq.

"As everything which brings a phenomenon within the reach of a larger number of observers is useful, you may, perhaps,

think it worth while to mention in the *Monthly Notices* that I find the dusky ring of *Saturn*, which I believe has only been seen by rather large telescopes, is discernible with much smaller instruments; and that the possessors of such may look for a sight of this interesting appendage to the planet with every chance of success. On the night of Thursday, 20th October, the slate-coloured interior ring was beautifully seen in my telescope of only 4 feet focal length, with an aperture of $3\frac{1}{8}$ inches. The eye-piece was an Huyghenian one, giving a power of 480. Not anticipating that the telescope would show it, I had not looked for it, but had my attention drawn to it by the inquiry of a friend, 'Why half the space between the ring and the planet was lighter than the rest?' Upon looking myself, I immediately recognised the dusky purplish light and well-defined inner boundary of the ring in question, occupying about a third of the space from the bright inner ring, and also saw its dark line across the body slightly broader at the ends than in the middle. Other persons also verified the observation. This was between 10 and 11·30 G.M.T. The atmosphere was excessively moist, and the sky rather hazy; but I have never seen the detail of the moon and *Saturn* to greater perfection than on that evening. I do not suppose that many telescopes so small as my equatoreal will show this ring, as my object-glass (by Ross) is a very fine one; but I should think good 5-feet telescopes, of $3\frac{1}{4}$ inches aperture, which are in the possession of so many amateurs, would."

On Rating Chronometers by Lunars. By H. Toynbee, Esq., Commander of the Gloriana, East Indiaman.

In vol. ix., No. 7, of the proceedings of the Royal Astronomical Society, dated May 11, 1849, will be found the notice of a paper "On the Practicability and Advantages of obtaining a Sea-rate for a Chronometer," which is published entire in the *Nautical Magazine* for February 1850.

In the paper alluded to, I endeavoured to prove how a good practical observer of lunars could obtain a better sea-rate for his chronometer than that supplied by the maker, and than one determined by other means during the ship's stay in port, also an original error which very rarely gives an error on Greenwich mean time of more than twenty seconds.

A general opinion seemed to be that I was correct, so far as my own observations were concerned, but that lunar observers generally could not apply the method satisfactorily.

My present object is to prove this objection erroneous. Late in 1850 I took the command of another ship, my chief officer during this voyage, on account of the delicate state of his health, was prevented from taking lunars.

In 1851, Mr. J. W. Quihampton (now commander of the Tudor) became my chief officer, and continued so until the middle of September 1853. He had been in the habit of taking lunars, but, like almost all persons with whom I have conversed on the subject, had been taught that the best lunars could not be depended upon

within fifteen miles of longitude, so did not think it possible to rate chronometers by them.

This opinion (which I believe to be almost universal amongst seamen) must be set aside, and the method, which is fully explained in the *Nautical Magazine* for February 1850, fairly tried. Mr. Quihampton did so, and I proceed to give a comparative view of the errors and rates of Dent's 1759 chronometer, found by his observations and my own, together with those by the gun at Madras, the ball at St. Helena, and the maker on arriving in England.

	Mr. Quihampton's Original Error. Fast of G.M.T.	Daily Rate. Gaining.	My Own. Original Error. Fast of G.M.T.	Daily Rate. Gaining.	Various Sources. Original Error. Fast of G.M.T.	Daily Rate. Gaining.
	m s	s	m s	s	m s	s
May 14	⊙ E. ☾ 38 19·3		⊙ E. ☾ 39 22·3			
	⊙ W. ☾ 40 10·7	8·9	⊙ W. ☾ 39 31·1	9·4	Flash of Madras Gun.	
	Mean 39 15		Mean 39 26·7		39 22·7	9·3
30	⊙ W. ☾ 42 43·4		⊙ W. ☾ 42 19·1			
	⊙ E. ☾ 41 18·9	9·5	⊙ E. ☾ 42 55·4	10·5		
	Mean 42 1·1		Mean 42 37·3			
June 12	⊙ E. ☾ 43 17·3		⊙ E. ☾ 45 4·0			
	⊙ W. ☾ 44 34·4	9·1	⊙ W. ☾ 44 20·1	9·9		
	Mean 43 55·8		Mean 44 42·1			
July 2	⊙ W. ☾ 47 34·4		⊙ W. ☾ 46 49·5			
	⊙ E. ☾ 46 16	9·0	⊙ E. ☾ 47 6·3	7·6		
	Mean 46 55		Mean 46 57·9			
13	⊙ E. ☾ 47 45·1		⊙ E. ☾ 48 23·3			
	⊙ W. ☾ 48 45·8	8·1	⊙ W. ☾ 47 54·7	7 0	St. Helena Ball.	
	Mean 48 15·4		Mean 48 9		47 54·5	
30	⊙ W. ☾ 51 6·9		⊙ W. ☾ 50 7·2			
	⊙ E. ☾ 50 9·5	8·3	⊙ E. ☾ 50 47·8	7·9		
	Mean 50 38·2		Mean 50 27·5			
Aug. 10	⊙ E. ☾ 51 50·7		⊙ E. ☾ 52 21·3			
	⊙ W. ☾ 53 2·9	9·2	⊙ W. ☾ 51 52·3	8·5	Maker's Error brought back by Maker's Rate.	
	Mean 52 26·8		Mean 52 6·8		52 9·5	
					Maker's Error and Rate.	
15	.....		.....		56 47·5	8·0

Note.—Each date applies to all the results on the same line with it.

We were only one day in St. Helena, so could obtain no rate by the ball. The ship arrived in the East India Docks on the 13th September, the chronometers were taken by the maker on the 14th, and the daily rate gradually decreased to 6ˢ·5 during the first *five days.*

On referring to the above table it will be noticed that in Mr. Quihampton's case, the ☉ W. ☾ lunars invariably give the greater, but in mine almost as invariably the lesser error. This may arise from a constant error in the observer, or his instrument; but the means of our ☉ E. ☾ and ☉ W. ☾ errors nearly agree.

The differences between our rates found at the same time, the mean of which is eight-tenths of a second, are reduced by taking a mean between the new rate and the one previously found. In cases where the chronometers, by their second differences, prove themselves to have been going regularly since the previous rate was obtained, I strongly recommend this to be done.

Since leaving Madras, Mr. Quihampton has rejected three sets of sights, and I two, they were deemed but indifferent observations before working them. In several instances lunars were obtained on only one day of the sun and moon's continuing in distance.

Both sextants were by Cary. Mr. Quihampton always used green and red shades, I a blue one; the same shades were always used, and the instruments placed as near as possible in the same state for each observation, as more fully explained in my first paper.

We each took and worked two sets when practicable, to check errors of calculation. They were both taken at the same sitting. Mr. Quihampton's set consisted of three, mine of five, sights.

The first part of the voyage was to Port Phillip, where the chronometers were not rated, still we found on arriving at Madras that by applying the lunar errors we had the Greenwich time within a few seconds.

During the passage from Madras to London, we had a furious gale off the Cape on June 23, 1853, and the second difference of the chronometers decreased twelve seconds before arriving at St. Helena, still we hove to at 4 A.M., July 12th, and at daylight found the island bearing north-west ten miles as expected.

Early on the morning of Sept. 8th we sighted the Eddystone lighthouse, when the cross-bearings and sights computing with the lunar error, placed us in the same position to a mile and a half.

I think the preceding table shows why lunars are not considered correct within fifteen miles of longitude, because most of Mr. Quihampton's errors, taken separately, give the longitude that much wrong. It was the same with mine until the sextant was carefully adjusted by the maker.

I think it has also proved that there is a constant character in lunars taken by the same observer, using the same instrument in the same state; whereby any person having practised this method for a short time, may readily show how much a single set of lunars gives the Greenwich time in error. For instance, Mr. Quihampton's ☉ W. ☾ and ☉ E. ☾ lunars differ in the Greenwich time about one minute and twenty seconds; now the half of this, or forty seconds of time, may be called his constant error; mine differ about twenty-nine seconds, so that my constant error is fifteen seconds of time, which applied to any single set of observations will give the

Greenwich time very nearly correct: hence an observer having ascertained his constant error, has the true longitude from a single set, taken either ☉ E. ☾ or ☉ W. ☾.

This voyage my constant error has been very nearly the same throughout. Last voyage the ☉ E. ☾ lunars gave eighteen seconds more subtractive error than the ☉ W. ☾ on the passage out to India, and thirty seconds less during the passage home. I am of opinion that this difference arose from a change in the state of the sextant during our stay in port.

What I consider to be new in this method is, its enabling an observer of lunars to find rates for chronometers each fortnight of his voyage, and so to mean ☉ E. ☾ and ☉ W. ☾ lunars—though several days have elapsed between the observations—that he obtains an original error for chronometers, which is very near the truth, and a constant error for himself, by the application of which a single set of lunars gives the true longitude. I have practised the method for nearly five years, and it has never misled me in determining the ship's position. My knowledge of the general accuracy of other observers convinces me that they may do the same, which, I think, this paper proves.

It cannot be too generally known that lunars a month old are very valuable. I have the calculations from which my errors and rates are deduced. They are at the service of any person wishing to see them.

Report of the Radcliffe Observer to the Board of Trustees, July 1, 1853.

The principal event reported upon is the completion of the electric communication between the Radcliffe Observatory and the Royal Observatory of Greenwich. The difference of time between the two Observatories has not yet been determined.

Since the last meeting two volumes of the Radcliffe Observations have been published, which bring down the published records of the Observatory to the end of 1851. The Observations for 1852 are stated to be ready for the press.

The calculations for the Catalogue of Circumpolar Stars, which were committed to Mr. Luff, have been brought down to the period of the last published volume; and it is expected they will be altogether completed before the next meeting of the Board.

Mr. Johnson proposes printing the Catalogue separately as a supplement to the fourteen volumes of Observations, of which it will contain the substance, retaining the same form and type.

The Heliometer continues to maintain its high character. Two years' experience has pointed out certain minor faults, which Mr. Johnson believes to be remediable; but he is waiting the completion of a long series of observations on which he has been engaged before he communicates with the maker on the subject. Such is the power of the instrument, that he believes a change of

relative position of two stars, marked by the $\frac{1}{4000}$th part of an inch on its scale, would not be undetected.

Steps have been taken for the construction of a Photographic Barometer and Thermometer. Mr. Johnson hopes, at the next meeting, to be enabled to report a considerable improvement in the meteorological department of the Observatory.

The Report concludes with some suggestions relative to the internal arrangements of the Establishment.

Remarks on the Erection of the Time-Ball of the Royal Observatory, Edinburgh. By Professor Piazzi Smyth.

The simplest principle of a time-signal is to let fall some sort of conspicuous body at the appointed instant. To be seen at a distance, it must be large; to be equally visible from all directions, it must be spherical; and to be well seen, projected as it will always be here on the sky behind the lofty Nelson's Monument to all observers except such as may be on higher ground still, it requires to be painted black. Again, that it may always descend with equal velocity, whether the day be calm, or whether there be much wind pressing the ball sideways against its supports and retarding its descent by friction, it must be heavy; and, in order that the observation be of as exact a nature as possible — in order that the instant of the *beginning* of the descent may be observed with the utmost accuracy — there must be a cross staff on the top of the mast, up to and in contact with which the ball is to be hauled, and the instant of separation of the ball in its descent from the said cross staff indicates the moment of the *beginning* of the fall with abundant precision.

We have thus a large and heavy ball (say four feet in diameter) hauled up to the top of a mast with cross-bars on the summit, and made to fall at a certain instant daily. That is the simple principle; but what takes place in practice? Firstly, such a ball falling a great height would soon break itself to pieces; and the stronger it is made, the heavier it must be, and the more suicidal force it will have. Secondly, the effect would be also that of a ramming engine to destroy the structure whereon the ball fell, in the present case the many-storied tower of Nelson's Monument, with all its primitive architectural decorations.

The earliest signal-balls which were made, though provided with ropes passing over pulleys by which they were enabled in their descent to raise a series of weights in order to check in a gradual manner the velocity of their fall, were yet invariably found, after a short time, to pull or to smash themselves to pieces. Steel springs were next tried to break the force of the concussion, but were pretty sure to be themselves snapped with a heavy ball, while a light one would not descend quick enough on a windy day. Recourse was finally had to compressed air, a spring of perfect temper

never injured by time, and capable of any degree of delicacy at first, and any amount of violent resistance at last.

To carry out this principle a staff was attached to the ball below, terminating in a piston, which in the course of its descent entered an accurately turned cylinder, and compressing the air therein, was gradually brought to rest. Were the cylinder quite closed at the bottom, the spring of the included air might be greater than required, and also have a tendency to throw the ball up the mast again, which would be somewhat troublesome to observers. But by simply opening a graduated aperture below, so as to admit of the air partially escaping as it is compressed, the strength of the spring is diminished, and by the time that the piston has descended to the lowest point, there is so little air remaining in the cylinder, and it is still escaping so fast, that there is no power left to make the ball rebound.

Thus the time-ball is made to descend without injuring the building or spoiling itself; and the trigger apparatus, by which the detent, that holds the ball when hauled to the top of the mast, is unlocked, being very nicely adjusted, and observers being duly cautioned to look to the instant of *separation of the ball from the cross-staff*, the descent — *i. e.*, this first part of it — is as instantaneous as need be. In the next place, the trigger being pulled, not by the finger of a person at the ball, but by an electro-magnet which is instantaneously set in action by the contact made at the end of a wire led into the walls of the Observatory, and brought immediately before the transit-clock itself; the instant for the signal outside can be conveyed to the undeviating mechanism there with all the refinement of a chamber experiment, and to the utmost extent of the observer's knowledge of the real time by the stars, as obtained the previous night, and continued on by the clock.

When the weather permits the stars to be observed, the time may be depended on to less than one-tenth of a second. But when, as too frequently occurs in this country, with its cloud-diversified sky, more attractive to the painter than suitable to the astronomer, no star may have been seen for several days, the clock may deviate considerably; nay, so much, that on such days it would be better not to drop the ball at all, rather than to give an erroneous indication.

But here, again, Science affords her assistance. Such unmitigated bad weather seldom prevails equally in the south of England and over this city at the same moments; consequently an observation is often possible, and can be well obtained at Greenwich, when quite out of the question here. Then by refinement of galvanic agency the instants of such an observation, the moments of the star being on the fiducial lines of the Greenwich transit, may be flashed along a wire and indicated on any other clock, as that of our own Observatory; if only there be complete wire connexion the whole of the way. Such a road for the electricity there has long been from the Greenwich Observatory to the railway station in Princes Street; but there being none thence to the Calton Hill, it could not be used for exact purposes. Within the last few days, however, the

Government has consented to the wire being brought up to the Observatory, on the strong representation of the importance of the measure by the Board of Visitors of the Royal Observatory, under the presidency of the Right Hon. Lord Rutherfurd, who, we may add, had long since evinced the warmest interest in the subject of the time-ball, and, as Lord Advocate, three years ago, was the important turning point of the whole scheme.

Nearly every contingency seems thus to have been prepared for, except, perhaps, the conveniency of the method of hauling up the ball, as influenced by the peculiarities of the site—viz. the apparatus being on the top of a lofty building, and at a distance from the Observatory.

With reference to this object, it may be remarked, that, some trouble might be experienced in ensuring a porter's daily attendance throughout the year with perfect punctuality, unless recourse is again had to modern science. In accordance with this, a plan has been proposed, by which a weight having been wound up at *any previous hour of the day or night*, then on electrical contact being made at the Observatory by the astronomer at a precise moment, that weight is unlocked, immediately descends, and hauls up the the ball. Next at five, or any other number of minutes, a second contact being made on another wire lets the ball down.

Thus the ball is both raised up and let down by delicate signals given from the interior of the Observatory by the astronomer himself with equal punctuality, both operations being similarly independent of any external person at the time. Hence arises a result not unfrequent when human labour previously employed in the mere brute purposes of raising weights or turning wheels is replaced by scientific machinery, that the work is not only cheaper but better done than before. For the ball may in this way be raised so quickly and certainly, that the beginning of its rise becomes as good a time-signal as the commencement of its fall. There may thus be two signals daily; two also which can never be mistaken for each other, as two successive descents might often be.

Report of the Syndicate appointed to Visit the Observatory of Cambridge.

In this Report an account is given of the labours of Professor Challis at the Observatory of Cambridge during the year 1852, and also from the commencement of the present year down to the 20th of May.

The first part of the Report relates to the observations made in the year 1852. The transit observations made during the course of the whole year amounted to 2050, and the circle observations to 1543. Of these, 362 transits and 340 circle observations relate to bodies of the solar system. The remaining observations of stars were made for the most part in pursuance of Prof. Challis's plan of forming gradually a catalogue of zodiacal stars situated within 5° of the ecliptic, to the ninth magnitude inclusive.

Observations of seventeen of the newly-discovered planets and of three comets (Biela's, Encke's, and Westphal's) were made in the course of the year with the Northumberland equatoreal. The comparisons in right ascension amount to 682, and those in north polar distance to 678. Eight occultations of fixed stars by the moon were observed.

At the beginning of the present year Professor Challis was induced to modify the system of meridian observation hitherto pursued by him, having found it to be no longer practicable to observe on the meridian with sufficient attention the large and constantly increasing number of newly-discovered small planets. The observations with the meridian instruments are now confined to those of the sun and moon, while the newly-discovered planets and comets are observed exclusively with the Northumberland equatoreal; the former more especially, in those positions wherein it is impossible to observe them on the meridian with the transit circle of the Greenwich Observatory.

The transit observations taken from the beginning of the current year to May 20 were 795, and the circle observations 553. In the same interval the Northumberland equatoreal was employed in observations of *Calliope*, *Lutetia*, *Thalia*, *Hygeia*, *Parthenope*, Secchi's Comet, *Iris*, and *Victoria*, the comparisons of these bodies with stars in right ascension amounting to 298, and those in north polar distance to 296. Twelve occultations of stars by the moon have also been observed.

Professor Challis has applied Mr. Pritchard's invention for getting rid of the oscillations at the surface of mercury, which occasion so much trouble to astronomers in observations by reflexion. The tremor is found to be much less than in a wooden trough. Further trial, however, is necessary before the new trough can be pronounced to answer the intended purpose in all respects.

The Report concludes with an allusion to the operations for determining the longitude of the Observatory of Cambridge by means of galvanic signals.

Tables of the Moon, &c. arranged in a form designed by Professor Benjamin Peirce, under the superintendence of Charles Henry Davis, Lieut. U.S.N., and published under the authority of the Hon. John P. Kennedy, Secretary of the Navy. Washington, 1853.

A review of the Lunar Tables constructed in successive ages presents an interesting picture of the progress of astronomers in unfolding the complicated irregularities of the moon's motion. It is a remarkable fact that the most considerable inequalities in the longitude of this body due to perturbation were satisfactorily established, not only previous to the discovery of the theory of gravitation, but even before any conception had been formed of the elliptical movements of the various bodies of the planetary system. It was by the aid of the much-decried theory of epicycles that astronomers were enabled to arrive at these important results.

Thus the discovery of the evection by Ptolemy was the direct consequence of a comparison which that astronomer instituted between the observations of the moon in quadratures and the corresponding places assigned by the epicyclical theory. Again, in modern times, when Tycho Brahé succeeded in bringing practical astronomy to a state of excellence hitherto unknown, the same theory was instrumental in conducting that astronomer to the discovery of the variation and the annual equation.

The elliptical theory which forms the basis of the famous Rudolphine tables, published by Kepler in 1627, was not applied very successfully by that astronomer to the moon. In representing the equation of the centre, he, indeed, experienced no difficulty; but his explanation of the evection was very unsatisfactory, being, in fact, a remnant of the epicyclical theory engrafted upon the elliptic orbit of the moon. Horrocks first showed that the evection might be completely reconciled with the elliptical theory, by supposing the eccentricity of the lunar orbit to be variable, and ascribing a libratory motion to the line of apsides. The Lunar Tables by Flamsteed, inserted in Moore's *Mathematics*, published in 1681, may be considered as the most complete embodiment of all the facts relating to the moon's motion that had been established by astronomers previous to the publication of the *Principia*. It may be remarked, that to Flamsteed is also due the merit of having first given the right explanation of the annual equation, the origin of which had hitherto been imperfectly understood by astronomers.

Newton, by his discovery of Gravitation, was enabled to establish the existence of several inequalities in the moon's motion, which had hitherto escaped the notice of astronomers. It is well known that his *Theory of the Moon* was not published in the original edition of the *Principia*. It first appeared in the year 1702, in the *Astronomiæ Elementa* of David Gregory, and was subsequently inserted in the second and third editions of the *Principia*. Four new inequalities in the moon's longitude were assigned by him as deducible from the theory of Gravitation. This made the whole number of the inequalities in the longitude now to amount to eight.

Newton's lunar theory continued for several years to form the basis of a succession of lunar tables, which were constructed by various individuals both in England and on the Continent. In 1749, Halley's Astronomical Tables, which had been printed off as early as 1719, were finally published. The lunar tables were similar to those which had been already calculated from Newton's theory, except that one of the four new equations was altogether omitted.*

The purely analytical researches of Euler, Clairaut, and D'Alembert, on the lunar perturbations, had the effect of introducing into the theory of the moon's motion a multitude of minute inequalities in addition to those discovered by Newton. Tables based on their respective theories were constructed by these geometers. Euler's

* Viz., the equation $2' 20''$ sin $(\mathbb{C} - \odot)$. The value of the coefficient of this inequality, according to Plana, is $2' 2''{\cdot}1$.

Tables were first published in 1746 (*Opuscula Varii Argumenti*, p. 137), and the theory upon which they are founded in 1753. Subsequently there appeared the results of his more mature researches on this subject, contained in a ponderous quarto volume (*Theoria Motuum Lunæ, Nova Methodo pertractata*, Petrop. 1772). New tables were given by him on this occasion, constructed upon a plan altogether novel, and which was suggested by the peculiar form of his theory. The moon's place was expressed by means of rectangular co-ordinates referred to the ecliptic and a plane perpendicular to it. When these were once calculated from the tables, it was easy to deduce from them the longitude, latitude, and radius vector. Clairaut's Lunar Tables were first published in 1752 (*Théorie de la Lune*, St. Petersbourg.) A revised edition subsequently appeared at Paris in 1765.* D'Alembert's tables were published first in 1754 (*Récherches sur différents Points du Système du Monde*), and afterwards in 1762 (*Opuscules Mathématiques*, tom. ii.).

Notwithstanding the high position occupied in the mathematical world by the three geometers just mentioned, the lunar tables constructed by them did not possess such a degree of accuracy as might have been expected from the immense developement which the expressions for the moon's co-ordinates had acquired from their researches. This may be attributed partly to the imperfect state of the lunar theory, which, as a subject of analytical investigation, could only be considered as yet in its infancy, and partly to a want of practical skill on the part of the geometers themselves, in correcting the results of theory by comparing them with an extensive series of actual observations of the moon's place.

The Lunar Tables of the immortal Mayer were first published in 1753 (*Acta Reg. Soc. Gott.*, vol. ii.). A corrected copy was shortly afterwards sent to London for the consideration of the Board of Longitude, and at his death, in 1762, his widow transmitted to this country a third and still further revised copy. It is well known that the Board of Longitude adjudicated favourably on the merits of these tables, and that the widow of Mayer received on account of them a considerable sum of money from the British Government. They were published at London in 1770.

The number of arguments used in calculating the moon's longitude by Mayer's Tables was fourteen. This was one-third less than the number used by Clairaut. Still it was universally admitted that the Tables of Mayer represented the moon's place with greater accuracy than any which had hitherto been constructed. The physical theory upon which these Tables were founded was in all respects identical with that which formed the basis of the Tables

* In this edition of Clairaut's tables the number of arguments for the moon's longitude, including the argument for the reduction, is twenty-one. The following tribute paid by the author to the illustrious Lacaille is worth quoting:—"C'est feu M. l'Abbé de la Caille qui m'avait penser à employer une colonne pour le supplément du nœud, au lieu d'employer celle du nœud même, ainsi que la distance de la lune au pole de l'écliptique à la place de la latitude." (*Théorie de la Lune*, p. 115.)

constructed by Euler, Clairaut, and D'Alembert; but their author was enabled to diminish considerably the number of arguments by means of a transformation suggested naturally enough by his analytical researches. In the expression for the moon's true longitude, as *directly* deduced from theory, the arguments of the various inequalities were formed by a series of combinations of the epochs and *mean* motions of the sun and moon. It occurred to Mayer, however, that as the *mean* longitude of the sun enters into the expressions for the perturbations of the co-ordinates of the moon's place only in so far as it depends upon the actual position of the sun, which is the disturbing body, the number of inequalities in each case might be considerably diminished by employing equivalent expressions depending on the *true* longitude of the sun. In the Tables of that astronomer the true longitude of the sun is accordingly used, instead of the mean longitude, as in those of Clairaut. Another peculiarity of these Tables, the necessity for which was occasioned by the substitution of the true for the mean longitude of the sun, consisted in the circumstance that after the first ten equations were calculated, the moon's mean longitude in the remaining arguments was successively corrected by adding to it the sum of the preceding equations.

The Lunar Tables of Mayer, on account of their accuracy and the comparatively small number of arguments which they contained, were generally employed for calculating the moon's place in preference to any others which had been hitherto constructed. The use of them, however, was attended with two obvious disadvantages. In the first place, it was indispensable as a preliminary operation to determine the sun's true longitude by means of the solar tables. Again, since the arguments did not vary proportionably to the time, it was impossible to accomplish their *direct* formation by means of tables prepared beforehand.

In 1780, Mason's Tables of the Moon were published. In their construction and arrangement they resembled Mayer's tables; but the coefficients were slightly altered, and eight small equations were added to the longitude which Mayer had expressly omitted on the ground of their being insensible. The number of arguments employed in calculating the moon's longitude amounted, therefore, to twenty-two. These Tables were afterwards published by Lalande in the second edition of his Treatise on Astronomy (1792).

In 1806, Bürg's Lunar Tables were published under the auspices of the French Bureau des Longitudes. Their mode of arrangement did not exhibit any novelty. The eight new equations added to the longitude by Mason were found by Bürg to be indispensable. To these he added other six very small equations. The entire number of arguments employed in the calculation of the moon's longitude now amounted, therefore, to twenty-eight. Of these, the first twenty-four were calculated independently, and then, as in Mayer's Tables, the mean longitude of the moon in the remaining arguments was successively corrected by adding to it the sum of the preceding equations.

Soon after the publication of Mayer's Lunar Tables, the inconvenience attending the formation of the arguments had suggested the desirableness of transforming the expressions for the moon's co-ordinates, assigned by that astronomer, into analogous expressions depending upon *mean* arguments, with a view to the construction of new tables. In the *Memoirs* of the Berlin Academy for 1781, Schulze has, by a most laborious process, effected the transformations necessary for this purpose. It does not appear, however, that any further progress was made in the construction of tables adapted to this form of the arguments.

Another important disadvantage connected with Mayer's Tables, and all others constructed upon the same plan, consisted in this, that they did not admit of being compared directly with theory. In order to the attainment of this end, it was indispensably necessary either that the arguments of the tables should undergo a transformation similar to that which Schulze had effected for Mayer's tables, or that the expressions for the moon's co-ordinates assigned by theory should be reduced to the form employed in the construction of the tables. In the *Mécanique Céleste* (tome iii. p. 275, *et seq.*), Laplace has transformed the arguments employed in the Tables of Mason and Bürg into mean arguments, for the purpose of comparing the results with his own theory, and in the *Monatliche Correspondenz*, 1804, p. 232, Bürg has effected the converse of this process. In the *Connaissance des Temps*, for the year XV. of the Republic, Burckhardt has transformed Laplace's formula for the moon's parallax into an expression depending on the true longitude of the sun, and compared the results with Mayer's Tables. Mr. Adams, however, has recently shown that no reliance can be placed on the conclusion which Burckhardt deduced from this comparison.

The publication of Carlini's Solar Tables, which originally appeared in the *Milan Ephemeris* for the year 1810, marks an important epoch in the construction of Astronomical Tables in general. In these tables the arguments consist of a series of arcs, varying uniformly with the time, and their values are expressed not in space but in *time*, the cycle of their values being represented, in each case, by the *period* of the inequality. Hence, if it was required to calculate the place of the sun for any assigned date, the tables furnished the values of the arguments for the nearest epoch, and then their values for the actual date were obtained *by adding to each of the tabular values the common interval which elapsed between the epoch and the date*. The simplicity of such a process is obvious at first sight, more especially if there should be a great multitude of arguments of different degrees of magnitude. Carlini suggested the advantages of this form in the construction of lunar tables, and he pointed out some of the most important modifications which would be necessary in that case in order to ensure its practical utility.

In 1812, Burckhardt's Lunar Tables were published. Although constructed according to an artificial form, like those of Mayer, Mason, and Bürg, they differed materially, in several respects from

any of the latter. The *mean* longitude of the sun was used instead of the *true*, and the arguments were expressed in terms of the centesimal division of the circle. The number of arguments in the moon's longitude was thirty-six. Of these, thirty-two were formed independently, and the mean longitude of the moon in the remaining arguments was then successively corrected by adding to it the sum of the preceding equations. The calculations founded on these Tables are seriously vitiated by an important error in the expression for the parallax, which Mr. Adams has recently detected.

In 1824, appeared Damoiseau's Tables of the Moon, founded solely on his own theoretical researches. A new edition of these Tables was published in 1828, in which the centesimal division of the circle, which was originally used in expressing the values of the arguments and equations, was replaced by the sexagesimal division. Mr. Airy has justly censured this retrograde step.* The various inequalities are exhibited in the form in which they are deduced from theory. The number of arguments in the moon's longitude is forty-seven. Of these, thirty-two are formed independently. The remaining fifteen are compounded of these, and the values of the corresponding equations are found by means of tables of double entry.

The Tables of Damoiseau exhibit a return to the form of construction employed in the Tables of Clairaut. The process for calculating the moon's longitude is exactly the same in both cases, with the exception of being much longer as regards Damoiseau's Tables, in consequence of the greater number of arguments.

The Tables employed by Mr. Airy in the Reduction of the Greenwich Lunar Observations are a modification of Damoiseau's, suggested by the theory of Plana. The calculations of the moon's place that have been required for this great operation are the most extensive that have been hitherto executed by the aid of the lunar theory in its most developed state.

The American Lunar Tables are constructed from Plana's theory, modified by the results of the more recent researches of Airy, Hansen, and Longstreth. The plan of construction is that recommended by Carlini. First of all, the date for which the moon's place is to be calculated, is expressed in mean solar days, counting from the beginning of the Julian Period. If it falls anywhere between 2300000 and 2400000 days (or nearly between 1585 A.D. and 2131 A.D.), the tables at once assign the values of the epochs and arguments for the nearest thousand of days, and the number of days which remain over the thousands is then placed under each argument. The sum of these two quantities constitutes the value of the argument for the date, except in a few instances, in which a small correction for secular variation requires to be added. As the value of any argument thus found frequently exceeds many complete periods of the inequality to which it refers, tables are given containing a series of multiples of the period, so that the nearest multiple being subtracted from the gross value of the argument as already found, the remainder, being less than a whole period, is immediately

* Reduction of the Greenwich Lunar Observations, Introduction, p. lxxi.

available for ascertaining the value of the inequality, by means of tables adapted to the successive values of the argument.

Tables are given for computing the values of the arguments in those cases wherein the date does not fall within the limits above mentioned. In such cases, as might naturally be expected, the process is not so simple.

In the tables which assign the relation between the arguments and the equations, the values of the latter are generally given for every hundredth of a day of the argument. It is but right to state that Carlini distinctly pointed out the necessity for this subdivision of the day in the application of arguments of time to the construction of lunar tables, and also recommended the use of tables of multiples of the periods for the purpose above mentioned. (*Effem. Mil.* 1810.)

The simplicity of the process for forming the arguments cannot fail to constitute a strong ground of recommendation in favour of these Tables. For the calculation of an ephemeris of the moon's place, they seem to be especially adapted. At the same time it must be admitted that the immense number of arguments, amounting to seventy-nine for the longitude, presents a serious obstacle to their practical utility. It may be remarked, also, that the recent researches of Mr. Adams on the lunar theory have tended seriously to invalidate more than one of the elements employed in their construction.

The arrangement of these Tables is stated to have been the result of a plan devised by Professor Peirce. It is very clear and masterly, and is in every respect worthy of that eminent mathematician. It may be mentioned, in conclusion, that the work, like most scientific publications which emanate from the American press, is beautifully got up, far surpassing in this respect any Astronomical Tables which have hitherto been published.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIV. *December* 9, 1853. No. 2.

G. B. AIRY, Esq., in the Chair.

Lieut. E. D. Ashe, R.N.,

was balloted for and duly elected a Fellow of the Society.

Discovery of a New Comet. By M. Klinkerfues.

On the morning of Dec. 2, M. Klinkerfues discovered a new comet in the constellation of *Perseus*. The time was too short to determine with certainty its position with respect to the neighbouring stars. The following is given by him as its approximate place:—

1853.	Göttingen M.T.	R.A.	Decl.
	h m s	h m s	° ′ ″
Dec. 2	16 3 49·5	1 37 19·5	+51 36 37·6

M. Klinkerfues estimates the daily motion in right ascension at — 30′ of space, and the daily motion in declination at — 90′.

The following elements of this comet have been calculated by M. Klinkerfues:*—

T	1854, Jan. 9, 8251	
	°	′ ″
π	.....	53 22 54·6
λ	.....	227 23 1·8
i	.....	66 46 24·0
Log *g*	...	0·306940

Retrograde.

Observations of Comet III., 1853. By W. Bradshaw, Esq., Master and Commander of H.M. Troop-ship, Resistance.

(*Extract of a Letter to Rear-Admiral Sir Francis Beaufort, dated Bathurst, Gambia, Oct.* 8, 1853.)

"Having during the passage of the Resistance from Port Royal to the Gambia observed a comet, no account of which was given in any of the newspapers seen by me at Jamaica, I beg leave to forward for your information the following observations made on board:—

"August 22d. In lat. 24° 50′ N., long. 73° 25′ W., apparent time at ship 7·25 p.m., observed a comet bearing W.N.W., the tail

* Extracted from the circular of Professor Chevallier.

vestigate how far this supposition can be justified by theory and reconciled with the coincidence of the alleged tail with the star *Arcturus*.

If the tail of the comet had no curvature, it should, as viewed from the earth, coincide with a great circle passing through the sun and comet; but the angle formed by this great circle with an arc joining the comet with *Arcturus* would be the geocentric effect of a curvature. Now it may be remarked, that the projection of this great circle upon the plane of the meridian must be represented by a straight line when its intersection with the equator crosses the horizon, but will, in the present case, where the right ascension of the sun is less, but its declination greater, than that of the comet, be curved previously to that time with its convex side to the north, and subsequently to the north pole. The calculation of the effect of curvature may then be based upon the geocentric place of the comet, derived from observation, by computing the angle at the comet in a triangle, in which the sun's and comet's polar distance and the intercepted angle, or the difference of their right ascensions, are given; and by subtracting from its supplement that angle at the comet contained between the arcs joining the comet with the pole and with *Arcturus*, the remainder is the geocentric effect of curvature, and will be found 28° 12′, assuming the right ascension of the comet to have been $11^h\ 22^m\ 29^s$, and its declination $= + 6°\ 54'$. It is found that the angular distance of the comet from the sun is 8° 51′, and from the star 42° 23′.

Although instances of greater curvatures are known, it may be worth while to ascertain the amount of the actual curvature, or the angle at the sun between two lines from the sun to the comet, and from the sun to that point in the comet's tail which, viewed from the earth, covered *Arcturus*. But, as physical causes can only be assigned for curvatures taking place in the plane of the comet's orbit, we shall imagine two great circles meeting in the geocentric place of the sun, and in the heliocentric place of the earth, the planes of which intersect each other in the produced radius vector of the earth. In the circumference of the *first* of these two great circles, the plane of which passes through the real comet, are its heliocentric and geocentric places, at the point where two lines from the sun and from the earth through the comet terminate. This circle coincides with the above, passing through the sun and comet, made use of in our former calculation. In the circumference of the plane of the *second* great circle passing through *Arcturus*, this star is at the same time the geocentric place of that point in the axis of the comet's tail, which appeared to us to cover it, whilst the intersection of this second great circle with the comet's orbit is the heliocentric place of this same point, and the arc of the comet's orbit intercepted between the first and second great circle is the actual curvature of the tail.

For the calculation we have, in addition to the above-stated observed place of the comet, the choice of more arguments than are required, which may serve to check the calculations. For my calcu-

lation only two parts of the elements are introduced, viz. the comet's radius vector, which, so near the perihelion as well as the inclination, may be sufficiently relied on. I omit the calculation, which may be arranged differently. The geocentric effect of the curvature as well as the comet's angular distance from the sun and from *Arcturus*, resulting from this calculation, agree perfectly with what was found by the former proceeding. The actual curvature, or the angle at the sun between the comet and the point in question, I find = 22° 5′, and the absolute distance of this point from the earth I find 0·64947, which is all within the limits of probability, so that we need not hesitate to ascribe the beam of light seen first in the west and then in the south-west to the comet. But, in order to establish a connexion between the first apparition and ensuing phenomenon (zodiacal light or aurora borealis), we must follow the comet in the different positions of the first great circle with respect to the horizon of Hamburg, during the rotation of the earth; whence it becomes evident, that the point of its intersection with the equator sets in the true west, and that the amplitude of that point of the beam of light which crossed the horizon at $9^h\frac{1}{4}$ was 2° 58′ north of west. As long as the sun was between the comet and the lowest point of the great circle, elongated 90° from its intersection with the horizon, the beam was directed upwards; but after the comet's passing this point, the beam was directed downwards, and the phenomenon must disappear. It further follows that, under the above-mentioned circumstance of the sun's north polar distance being the lesser, the amplitude of the setting point of the beam must, until its disappearance below the horizon, have had a southerly motion, whilst the comet itself below the horizon was proceeding to the north. All this agrees perfectly with the observations in Hamburg and all other places, where, in general, the first phenomenon was seen more towards the south, where it was perceived later, setting aside anomalies arising from difference of latitude. At $9^h\ 15^m$ the

Sun's depression below the horizon of Hamburg was	19° 34′	azimuth	N. 43° 46′ W.
Comet's — — — — —	16 25	—	N. 52 25 W.

At 11 o'clock the depression of the comet was 26° 17′, and its azimuth N. 26° 38′ W., being then 1° above the sun. In more northerly latitudes the comet approached nearer the horizon, so that the apparition of its more expanded fan-like tail could, with the aid of increased darkness, produce a phenomenon similar to an aurora borealis. I beg leave to mention that Mr. Hartnup, as I understand, has called it an aurora borealis of unusual appearance. Towards two o'clock in the morning of September 29 an aurora borealis was seen at Durham. Bruhn's comet was then quite near the horizon, 36° perpendicular above the sun. I have further to impress that the comet of Klinkerfues was *in its perihelion*, and the comet of Bruhn's and the great comet of 1843 near it.

There is, however, one circumstance which is more difficult to

reconcile with this hypothesis, that is its proper motion, differing from that of the fixed stars, and greater than could arise from its motion in its orbit. But, considering that the tail of a comet is a fluid which cannot be classed under what is generally termed heavenly bodies, its motion cannot be put to the mathematical test before its nature is better known. It is, moreover, well known that motion has been observed in the tails of several comets, and that it consists of rays which project in short intervals of time, considerably more or less, like those of an aurora borealis.

As these points appertain to physical astronomy, I must leave them to those who are better versed in that part of the science, and confine myself to the following remarks. The distance of the comet from the earth was too great to allow a contact of its expanded tail with the atmosphere of the earth. But, notwithstanding, the phenomenon might have been an atmospherical one caused by the tail of the comet, as there are perihelia round the sun and halos round the moon, rainbows, &c. &c., which expand and contract, and are generated in the atmosphere. And although there have been observed more auroræ boreales than comets, there certainly also exist more comets than those that have been discovered; and it has already been remarked by others, that our earth may often have been enveloped in the tail of a comet without our having been aware of it.

Extracts of a Letter from Professor C. Piazzi Smyth relative to Cometary Physics and to the Rating of Chronometers by Lunars.

"It is generally believed that the tails of comets are only put on near the perihelion point of their orbits; that they are produced by the intensity of the sun's rays there, and are dissipated on either side. Now I maintain that this is the reverse of what actually takes place. The comets have all the particles of their mass, whether tail, body, coma, &c., always with them; though in very different states of concentration at different times: and this it is which, combined with the greater or less intensity of solar illumination, according to their distance from the sun, and the greater or less angle under which they may be seen by reason of proximity to the earth, causes the variations in observed appearance. Moreover, it is now found that all comets, large and small, invariably become not only smaller, but more compressed and concentrated, as they approach the sun; and so far from a dissipation of solid matter, there is a bringing together of it, and a consequent brightening. The tail, indeed, shuts up like a many-draw telescope as it nears the perihelion, and opens out again as it leaves it. The amount of such concentration appears to vary in a proportion not yet accurately ascertained with the length of the radius vector or distance of the comet from the sun. So that, with such comets as Encke's and Biela's, where the difference of perihelion and aphelion distances is not very great, the amount of concentration is never considerable, and they are never

seen but as faint, vaporous, telescopic bodies. But when the aphelion distance is very great, and the perihelion distance very small, as with the comet of 1843, and to a much less extent with the third one of 1853, then so great a concentration occurs, there is such a bringing together of finely divided matter, that, though otherwise invisible, it is now able to reflect the sun's rays so strongly as to become abundantly apparent to the naked eye in the evening, and finally, at the perihelion passage itself, for a short time even in open daylight."

"I may further remark that, in consequence of the concentration of matter which ensues as the comet approaches the perihelion, much confusion arises from speaking of the length of a comet's tail without a qualifying note of the part of the comet's orbit at the time. The same reasoning applies to every other part of the comet, for it is all vaporous, but the nucleoid centre. Here, if anywhere, is the solid body, but it is infinitely small. The old writers do, indeed, talk of very large nuclei in former comets, and that of the present one has been described to have been, on August 27, upwards of 9000 miles in diameter, or enough to revive the old fears lest it should come in contact with the earth and break it to pieces. But that was not the nucleus, it was only the head round about it, that head being composed of the thinnest and lightest of conceivable vapour, merely rendered apparent there by the particles being brought very close together, and the whole so near the sun as to be lighted up by a more than ordinary blaze of light, and then to shine forth like a luminous object, as a thin cirrus cloud so often does when seen in the neighbourhood of the solar orb."

"Exactly the same appearance was observed at the similar part of its orbit in the great comet of 1843, when everything being on so much larger a scale, its internal economy could be better studied, and a powerful reflecting telescope actually showed the transparency, exhibited stars shining freely through that which persons with smaller telescopes were calling the solid nucleus. Day by day, too, as the comet left the perihelion, the once solid-looking head expanded and enlarged, and its outline became indistinct, and the whole was at length lost to all view, from being so rarified as neither to be able to reflect back any of the rays of the sun, nor to stop that coming through it from the smallest of the stars in front of which it passed."

* * * *

"As to the difference between the results of different lunars, Captain Toynbee should be advised of the probability of its arising from errors of the Tables; *i.e.* of the moon's place in the *Nautical Almanac*.

"For this amounts, in some parts of a month, to 15″ or 20″; and as it varies with opposite signs throughout the month, that is a reason why he finds lunars a fortnight apart, taken together, give a better result than two taken on following days.

"Again, if he can show that sensible errors to sailors arise from

the errors of the Lunar Tables in places in the *Nautical Almanac*, it will be a very strong argument for new Lunar Tables being made, based on all the recomputed Greenwich Observations, including the discoveries of Hansen, Adams, and others.

"Further, he should never depend on *Nautical Almanac* or anything else for the diameter of any heavenly body, but measure it for himself with his own instruments, and, comparing the result with the *Nautical Almanac* for the time, get the correction due to that cause: and always apply this correction in any case wherein the *Nautical Almanac* value must be used, as when the moon is near conjunction."

Remarks by Mr. Carrington on presenting to the Society printed Copies of Nine Circumpolar Charts.

"I have the pleasure this evening to present to the Society printed copies of nine draft maps, on a scale of an inch and a half to the degree, containing all the stars above the 11th magnitude, and many of the 11th within 9° of the north pole. These maps are the result of a first examination of the region I have selected for observation, and were formed principally with the object of making a provisional catalogue for my own use, during the second stage of accurate observation on the meridian. They have been entirely executed with my $4\frac{1}{2}$-foot equatoreal, described in the last *Monthly Notice*.

"The process followed in their construction was first to zone over the region in two broad zones, of which the first extended from 4° to $9°\frac{1}{2}$, N.P.D., and the second from 5° above to 5° below pole. From March 25th to May 20th of the present year, 20 zones were thus observed, which, when reduced and cleared of duplicates, yielded about 2000 net points for a foundation for the maps. The degree of accuracy attained was quite sufficient for the purpose. The stage of filling in was then proceeded with, and terminated on Nov. 8th. In each case a skeleton map was first prepared of the stars observed in the zones, and then filled in by estimation, considerable pains being taken to get satisfactory alineations. The map was then copied on a glass desk, through which the light of the sun was made to pass; and the copy was again compared with the heavens, a general revision and the adjustment of the relative magnitudes being chiefly attended to. It is the date of this second comparison which is given on the maps. From this corrected copy two copies were taken; one in common ink for my final standard copy, and one in anastatic ink for transfer by the printer, whose proofs were compared with and corrected by my standard copy.

"My printer was Mr. Rudolph Appel, of No. 43 Gerrard Street, Soho, and his charge for fifty copies of the same map, including cost of transfer and paper, twelve shillings. Believing that the anastatic process of printing has not been previously applied to the ready and cheap production of star maps, I beg to call attention to these maps, as specimens of what may be done, and to add at

the same time that the excellence of the result increases with the artistic skill of the author.

"I particularly wish it to be understood that I regard the present maps as imperfect, and merely as drafts, although tolerably complete as maps go, and well filled in. Any one who has worked on a map will have found, that to produce one which shall be satisfactory as a picture to the eye of another observer using a different telescope, it is very necessary for him to go over his map at different times, using different apertures. This has not, at present, been done for these maps. The sensible presence of twilight during the months of June and July has been another source of imperfection. For, as the nine maps are very nearly of equal spherical area, they ought to be very accurate guages of the polar region; but, owing to the cause named, and to the effect of thin fog on every night given to map No. 3, this relation is nullified for the present.

"I have distinguished by wings all those stars which are stated by Argelander in his *Uran. Nova* to be visible to the naked eye; by the letter D all those stars which, under a power of 28, appeared to consist of two components, too close to be represented together on the map; and by the letter N all places which appeared nebulous, and at which I should halt in sweeping for comets, although it is very possible that some of them may be clusters easily resolvable under moderate powers. Other stars I have in all cases distinguished by dots of different sizes; a method of representing magnitudes which, as more pictorial, I prefer to any other.

"I may mention, in conclusion, that during the formation of these maps I was soon struck with the frequency with which little meteoric sparks passed my field of view, often leaving behind little trains of light, and that, finding it an almost constant phenomenon, I took to noting down their paths on my working-maps as I proceeded. In this way I have collected 49 little paths of very various directions, but among which one may, perhaps, trace the existence of a little shoal crossing the pole in lines generally parallel to the meridian from 1^h to 13^h R.A. On the second night of the Laurentius stream this year, there was a marked increase in the number seen; seven were marked down, while others flew through the field too quickly to be caught. As I also perceived increased activity on several cold and windy nights, these minute telescopic meteors appear to consist of two classes, like their larger brethren, those of the naked eye, namely, one of a cosmical and one of an electrical origin."

Redhill Observatory.

On a Mode of Mounting a Telescope equatoreally.

By Mr. Joseph Hopkins.

"Having recently taken out a patent for 'a mode of obtaining a straight line parallel to the earth's axis, or of making the axis of a tube or the axis of a telescope parallel thereto,' which (amongst

other purposes) may be applied to the construction of sun-dials by which minute portions of time might be marked, or to the production of equatoreal motion in a telescope without the preparatory aid of levels or divided circles, and without the usual preliminary requirements of a knowledge of the latitude and direction of the meridian, I am anxious to lay before you the application of the principle to an instrument which may, perhaps, be not inaptly named 'the portable observatory.'

"Upon a common tripod telescope stand, a brass tube is so mounted as to have a horizontal and a vertical motion, each of which may be clamped so that the direction of the axis of the tube being once determined its position may be rendered permanent.

"The tube is 5 inches in length. If now at one end of it were inserted an eye-piece, in the centre of which was drilled a hole about one-tenth of an inch in diameter, and at the other end an object cover with a central circular aperture of the diameter of a quarter of an inch, the eye of an observer on looking through the smaller of these apertures would take in a field of view, the circumference of which would be equal to that of the circle which *Polaris* apparently describes round the true polar point, and if by the requisite horizontal and vertical motions these circumferences be made to coincide their centres will also coincide, and the axis of the tube will be parallel to the earth's axis.

"The coincidence here spoken of may be nearly enough obtained where a plain tube only is used, by viewing the pole-star on that part of the circumference of the circular diaphragm nearest to the constellation *Cassiopeia;* but, as in the instrument now brought before your notice, a small telescope is used, the axis of which is to be made parallel to the earth's axis, I shall proceed to show the mode of obtaining this parallelism.

"On the outside of the tube of the small telescope are two circular rings, which are carefully ground and fitted into collars in the brass tube: an axis of the telescope is then made to coincide with the common axis of these collars, and the centre of its diaphragm is made coincident with this axial line, the diameter of the diaphragm being such as to give a field of view to the telescope of $2^{\circ}\ 57'$ being double the north polar distance of *Polaris*.

"A brass sector, shown in the drawing, is made to move round the upper part of the brass tube at right angles to it, carrying an index and two small perforations, through which latter mentioned, when the stars ε and ζ *Ursæ Majoris* are seen by the observer, the eye taking in at the same time the field of view of the small telescope, the position to which *Polaris* must be brought on the circumference is pointed out by the line of light seen through the index.

"As a test of the correctness of its place thus found, turn the small telescope about its axis in the collars of the tube, when, through an entire revolution of the telescope, the star should continue to cling, as it were, to the same point of the circumference of the diaphragm.

"The axis of the collars of the tube being thus made parallel to the earth's axis, we remove the small telescope and sector, insert the axis, which is fastened by clips to the larger telescope, and fitted by rings of bell metal into the collars of the tube, attaching to it the right ascension circle with its clamp.

"It is evident that to whatever height the telescope be elevated, if it be turned about this axis, it will move in planes parallel to the equator; and if it be directed to a star, a motion given to it from east to west by the hand or by a hook's joint, will serve to keep the object in the field of view.

"I have chosen, however, to give motion to the telescope by the action of a floating syphon, the weight carried by the float being connected with the right ascension circle by a cord clamped in one of its grooves.

"The upper edge of this circle is exactly 24 inches in circumference, the lower edge 25 inches nearly, the former having a proportion to the latter equal to that of the sidereal to the mean lunar day. Each of these circumferences is grooved to the same depth. The thickness of the cord should be double the depth of the groove.

"The large cylindrical pan is about $18\frac{1}{4}$ inches in diameter: it contains in a depth of 12 inches 95·7 pints. This quantity of water should be run off by the syphon in 12 sidereal hours; the rate (which should be adjusted while the telescope is in motion) is obtained by raising or lowering the syphon by the rack and pinion, till it delivers 1 gallon in 1 mean solar hour, or 1 pint in $7\frac{1}{2}$ minutes.

"The rate thus adjusted, if the motion of a star is to be followed, place the cord in the upper — if of the moon in the lower — groove of the right-ascension circle, and let it hang by the small plummet over the pulley fixed to one of the legs of the stand.

"The telescope directed to the object, the cord clamped, and the syphon in action, no movement of the instrument will take place till the cord is so stretched as to overcome the friction of the collars in the tube; we, therefore, turn the milled head at the upper edge of the pan, which, by opening the stop-cock, causes the floating weight to sink rapidly; this motion being stopped when the object appears in the centre of the field of view, the action of the syphon alone will cause it apparently to remain stationary.

"I wish it to be distinctly understood that the instrument now submitted to your notice was designed to show (independently of its application to the production of equatoreal motion) the methods by which an extended wire, the axis of a tube, or the axis of a telescope, might be made parallel to the earth's axis to a sufficient degree of exactness to make it available for astronomical observations.

"Other simpler forms might be adopted where one telescope only need be used, or a telescope and its finder, the latter having a field of view of 2° 57′ might serve to determine the polar axis, and be so mounted as to produce the equatoreal motion required.

"Where very powerful telescopes are used, the smaller one used

for finding the polar axis might have a field of view of 3°, and the exact position of *Polaris* within the circumference might be determined by a micrometer adapted for the purpose; this would admit of the requisite corrections for refraction.

"Such a telescope attached in the same manner as a finder to the polar axes now used in observatories would serve readily to test the accuracy of their position, while in the Fraunhofer form of equatoreal it may be substituted for the polar axis, when we should obtain the advantage of making the axis of one telescope the fixed axis about which another may be made to revolve.

"As differences in the temperature of the water occasion some little variation of rate in the quantity delivered by the syphon, the rack-work should be graduated for these differences, or self-regulating clepsydras may be constructed either by using two cisterns and two syphons (one of the latter acting as a supply pipe to the cistern carrying the weight connected with the telescope), or if one cistern only be used by a thermometer carrying a piston attached to the syphon, the bulb of the thermometer being fixed under the lower surface of the float.

"If the telescope be not well balanced about its centre of gravity, more or less weight will be required to move it, as its direction is varied. If, however, the float cover a large surface of water, and the syphon, when adjusted for the mean temperature, be at some depth below this surface, no perceptible variation of rate will be occasioned.

"The action of the clepsydra produces a smoothness of motion not to be surpassed; and I feel assured it might be adapted to the requirements of an observatory, more especially as its rate may be so readily brought under the immediate control of the observer."

Description of a Miniature Observatory erected at No. 200 High Street, Exeter. (Latitude 50° 43′ 19″·45 N., and longitude $0^h\ 14^m\ 7^s$ W.) By Messrs. Ellis and Son.

(*Communicated by Dr. Lee.*)

This Observatory is built over workshops at the back part of the premises in the above locality, about 40 feet above the ground, and at 175 feet above the mean level of the sea, which elevation commands the northern horizon (48° below pole) as well as 36° of south declination. The upper chamber is 3 feet 1 inch wide (except that portion which falls into the return of the chimney, and which is a little wider), 5 feet 8 inches long, and 6 feet high. It has north and south doors, a chicket window, forming a convenient desk and cupboard, and a skylight, in the direction of the meridian, which is readily opened by means of a short lever. The eastern wall is painted; the others canvassed and papered. The doors and meridional opening are painted black. The roof being flat is covered with zinc.

The transit, a 20-inch instrument by Troughton, is mounted on

a bracket built into the eastern wall, and is as steady as can be expected at such a height in the midst of a crowded city.

A Dent's dipleidoscope, furnished with a telescope, is fixed to the same bracket.

A sidereal clock stands, or rather is hung, in the south-east corner, and an electro-magnetic dial, by Shepherd, occupies the space between it and the south door. The latter shows Greenwich mean time, and is connected with an electro-magnetic pendulum and another dial, in a distant part of the premises, on the ground-floor.

The thermometers, by Negretti and Zambra, have wet and dry bulbs. A patent maximum and a minimum thermometer are arranged on a stand outside the north window.

A Glaisher's rain-guage is erected 10 inches above the roof, and its cistern is also placed outside the north window.

A standard barometer, Barrow's, No. 25, is hung against the eastern wall of the lower platform, and is 164 feet above the level of the sea.

This paper was accompanied by a plan of the Observatory, and a few observations of Comet III., 1853, by Mr. H. S. Ellis.

Notes on the Appearance of the Zodiacal Light in the Spring of 1853. By T. W. Burr, Esq.

The atmosphere being exceedingly transparent, and the horizon clear round my residence at Highbury, I had since January kept watch for the appearance of the zodiacal light, but it was not until Friday, 11th March, that I was able to distinguish it. On that evening the sky was perfectly cloudless and the air calm. The sun and moon had both set by $7^h\ 30^m$ mean time, and the yellowness of the twilight having also disappeared, the desired luminosity became visible in the western part of the heavens. It was a very long cone or lenticular-shaped beam of nebulous milky light, starting from the western part of the horizon, but having more of its base to the north than south of that point. The angle made with the horizon was considerable, being about 45°, and the breadth at the base about 30°. These measures were only approximate estimations. The upper edge of the cone was less distinct than the lower, which was well defined. The point could be traced very nearly to the Pleiades, where it was lost. This group was then about three hours past the meridian, and about 50° in altitude. The point was not rounded off in the manner usually described and drawn, but sharper, so that the whole light looked much like half of the nebula in *Andromeda*, as drawn in Sir J. Herschel's *Outlines of Astronomy*. The stars α, β, and γ *Arietis* were immersed in the beam of light; *Saturn* shone brightly, just beneath its lower edge; and the principal stars of *Andromeda* were beyond the northern boundary. The light appeared generally to follow the line of the ecliptic, and to extend about 65° from the sun's place.

It was equal in brightness to the portion of the Milky Way in *Cepheus* and *Cassiopeia*, then visible; and its presence made a striking contrast between the western sky and the dark blue of the eastern heavens. The light was watched till $8^h\ 30^m$, when it was still visible, but much fainter, and not reaching beyond *Saturn*.

At the close of the Meeting the President stated that operations had commenced for determining the difference of longitude of Brussels and Greenwich by means of galvanic signals, with the view of thereby forming an electric communication between Greenwich and the principal Observatories of the Continent. Observations of signals and of transits had been already made for this purpose at Brussels by Mr. Dunkin of the Greenwich Observatory, and at Greenwich by M. Bouvy, one of the assistants of M. Quetelet, the Director of the Royal Observatory of Brussels. The Observers were subsequently to exchange positions, by which means it was to be hoped that all personal equation, both for the transit observations and for the observation of the signals, would be completely eliminated. With respect to the velocity of the electric current, the President remarked that, in the present instance, there was reason for suspecting it to be affected by the subterranean and submarine passage of a portion of the wire. It appeared that the time occupied by the electric current in passing from Greenwich to Brussels amounted to $\frac{1}{10}$th of a second, whereas the time occupied by the current in passing from Greenwich to Edinburgh, which was at almost double the distance, amounted only to $\frac{1}{17}$th of a second. The velocity on the American telegraphs is still greater. The President referred to experiments instituted by Mr. Latimer Clark, which, in company with Dr. Faraday, he had had the pleasure to witness; which fully supported the idea that the galvanic induction of the surrounding matter, without impairing the insulation (which is sensibly perfect), materially retards the progress of the galvanic current.

The President acknowledged the obliging conduct of the authorities of the European and Submarine Telegraph Company, who had cordially co-operated in promoting the success of this important undertaking.

Note respecting Capt. Toynbee's Paper on the Rating of Chronometers by Lunars.

In reference to the communication of Captain Toynbee, respecting a method for determining the rates of chronometers by lunar observations made at sea, which have appeared in our *Notices*, we may observe (what many of our readers are doubtless aware of) that in the ordinary application of lunar distances taken with a sextant, the longitude corresponding to each set of distances is computed. When, however, chronometers on shipboard

are sufficiently trustworthy, the ship likewise changing her geographical position, in this case it is found to be more expedient simply to register the respective errors of the chronometers from Greenwich mean time, as computed from each set of distances, with the day and hour of observation. When a number of such results are obtained, sufficient to remove as far as possible the unavoidable errors of instruments, as well as personal peculiarities in observing, and these results being at the same time confined within reasonable limits as regards date of observation, then a mean of the errors is found corresponding to the mean date. Thus new errors and rates for the chronometers are determined, as pointed out by Captain Toynbee.

We must observe, however, that this valuable mode of consolidating, as it were, the results of lunar observations when made at sea, and the ship changing her position, and which method Captain Toynbee has so skilfully and successfully applied to practice, is not new. In the well-known Nautical Tables of Mendoza Rios, 2d edition, p. 45 of the explanation to the tables, he will find the method clearly explained, with an example of its application; and M. Rios observes, "The preceding rule and example have been communicated to me by my esteemed friend Captain Huddard, who has successfully made use of that method in his voyages." The same method was extensively applied in the polar expeditions of Capt. Sir E. W. Parry, during the periods of summer navigation; and the longitudes of various places thereby determined. In his second voyage in 1821, 2, 3, it was somewhat modified, by considering the weight of each day's observation to depend upon the number of observations, and also the interval between that day and the date from which the new errors were estimated.

We have now before us a considerable collection of observations, made for the express purpose of determining the lunar rates of chronometers in the South Pacific, and other parts of the world. Also, a printed form, in use at the Naval Schools of Greenwich Hospital, having the same object in view. Discussions of this nature are not, however, without value; if they elicit nothing that is new, yet they bring forward more prominently, and give strong testimony to that which is sound and valuable; and it is gratifying to find officers using the talents and energy with which they are gifted, and *discovering for themselves* the best modes of obtaining results upon which the safety of their ships so much depends.

The *Nautical Almanac* for 1857 has just appeared. This is the first volume of the Ephemeris which has been published since the appointment of the new Superintendent, J. R. Hind, Esq. Besides the usual quantity of matter it contains an extension of the list of Fixed Stars from 100 to 147, and a Supplement containing Ephemerides of all the newly-discovered planets for the year 1854.

Of the forty-seven stars now inserted for the first time, the mean

places of forty-three have been derived from the Greenwich Observations of 1850, as printed, and the observations of 1851 and 1852, as supplied in manuscript by the Astronomer Royal. The positions of *γ Orionis*, h^2 *Sagittarii*, *ε Capricorni*, and γ^1 *Virginis*, have been taken from the Greenwich Twelve-year Catalogue, the place for 1840 alone having been adopted for the latter star. The proper motions, as determined by the Rev. R. Main, in his paper on the subject (*Mem. Ast. Soc.* vol. xix.), or computed by similar formulæ, have been included in the reductions of the mean places of the forty-seven additional stars to the year 1857.

Among minor improvements visible in this volume may be mentioned an economising of space, which has been judiciously employed in promoting the usefulness of the Ephemeris.

Determination of the Longitude of Papeété, from Observations of a partial Eclipse of the Sun.

(*Communicated by Rear-Admiral Sir Francis Beaufort.*)

The geographical position of Tahiti was originally determined by Captain Cook on the occasion of the expedition which was despatched under his command by the British Government to observe the transit of *Venus* at that island in the year 1769. The observations of the transit, which occurred on the 4th of June, 1769, were made at a place in the northern extremity of the island, since called Point Venus. The calculations indicated the longitude to be 151° 52′ 54″ relatively to the meridian of Paris.

The observations contained in the present communication were made at the Government House of Papeété (the capital of Tahiti), on the occasion of an eclipse of the sun, which occurred on the 6th of June, 1853. It was intended that observations should also be made at Afaite, one of the neighbouring islands, where the eclipse was to be annular; but the expedition despatched by the Governor for this purpose failed to arrive before the occurrence of the phenomenon.

The eclipse was observed at Papeété by three individuals, viz. by the Governor, by Lieut. Desaux of the French Navy, chief officer of his staff, and by M. Briot, his ordnance officer. Lieut. Boulangé counted the seconds for the three observers upon a marine time-keeper, regulated from observations made by him on the preceding days and two hours before the occurrence of the phenomenon. The sky was very clear.

The precise instant of the separation of the two discs was announced by the three observers as follows:—

	h	m	s
M. Briot	12	51	41·0
M. Desaux	12	51	43·5
The Governor	12	51	44·0

Of the three observations, the two last are entitled to the

greatest confidence, partly on account of their accordance and partly from the superiority of the instruments with which they were made. Their mean, viz. $12^h\ 51^m\ 43^s{\cdot}75$, or in mean time, after all corrections were applied, including the error of the watch, $22^h\ 7^m\ 40^s{\cdot}7$ (June 5), was accordingly employed as the basis of calculation, in which account is taken of the heights of the barometer and thermometer at the time of observation. The following are the observations from which the latter data were deduced:—

	h m	Bar.	Ther. ° ′	
June 5	2 30	0·764,8	26	(Centigrade)
6	2 25	0·764,02	26 3	—

The calculations for the determination of the longitude were executed by M. Adam Kutcyski. They give for the longitude of Papeété:—

	h m s
In Time	10 7 34·31
Arc	151 54 4·65

These numbers have reference to the meridian of Paris. They present a satisfactory agreement with the longitude indicated in the *Connaissance des Temps*. In the latter work the longitude of Point Venus is stated to be 151° 49′ 19″. Now M. Kutcyski, by a process of triangulation based upon this determination, found the longitude of the Government House of Papeété to be 151° 54′ 4″·5, which differs only 15 hundredths of a second from the result obtained by means of observations of the eclipse.

On the Parallax of Argelander's Star. By Prof. C. A. F. Peters.*

In this paper M. Peters subjects to a critical discussion the recent researches of M Wichmann on the parallax of Argelander's star (1830 Groombridge), and assigns what he considers to be the most trustworthy results deducible from the observations.

The basis of M. Wichmann's investigation consisted of a series of heliometric measures of the distance of Argelander's star A, from three other stars in its vicinity, a, a', a''.† The star a was on one side of the principal star, A, and the stars a', a'', were on the other, all the four stars being nearly upon the same parallel.

By a discussion of the observed *differences* of the distances of the stars of comparison from the principal star A, M. Wichmann found the parallax of the latter star to be 0″·135, with a probable error of 0″·013, the parallaxes of the stars of comparison being supposed to be insensible.

A similar discussion of the observed *sums* of the distances presented a peculiar difficulty in consequence of the maximum effect

* *Ast. Nach.* No. 865–6.

† The stars are here denoted as in Wichmann's Researches, for an abstract of which see *Monthly Notices*, vol. xiii. p. 131.

produced by the influence of temperature on the instrument, being almost coincident with the maximum effect of parallax. M. Wichmann was, therefore, obliged to adopt two distinct hypotheses in the conduct of this part of his investigation. By the first hypothesis the observed variations of the sums of the distances were supposed to arise entirely from the influence of temperature on the instrument at different seasons of the year; by the second hypothesis, Bessel's coefficient of temperature was introduced into the investigation as a definitive result, and the residual effect was attributed entirely to parallax. The first hypothesis gave for the coefficient of temperature a value greatly exceeding that which Bessel had deduced from his researches on the subject. By the second hypothesis, M. Wichmann found the parallax of a to be $1''{\cdot}17$, and that of A to be $0''{\cdot}71$, assuming the parallaxes of a' and a'' to be insensible.

M. Wichmann is of opinion that the last-mentioned value of the parallax of A ought to be regarded as the most trustworthy which has hitherto been deduced.

Bessel's investigation of the coefficient of temperature was founded on a series of heliometric measures of the distances of *Halcyone* from ten other stars of the *Pleiades*. These measures had been executed by Bessel, Schlüter, and Plantamour, during the interval comprised between the years 1830 and 1840. Denoting the value of this coefficient by ε, Bessel deduced from his researches, $\varepsilon = +0{\cdot}00000378$ with the probable error $0{\cdot}00000045$.

Dr. Peters found, however, that after Bessel's correction for temperature thus found had been applied to these observations, and they were in other respects completely reduced, the outstanding errors *varied with the season of the year*, and (taking into account the distances measured in both cases) were of the same order of magnitude as $1''{\cdot}17$, the value which M. Wichmann obtained for the parallax of the star a by an investigation of the *sums* of the distances of the comparison stars from the principal star A.

These variations could not be explained by the effect of parallax, since, although the stars of comparison lay in every direction round the standard star, the variations for each day of observation always took place in the same direction.

It was evident, therefore, that no reliance could be placed on a result of parallax, the value of which was determined by employing Bessel's correction for the influence of temperature on the instrument.

From Schlüter's measures alone M. Peters determined the value of ε to be $+0{\cdot}00001243$ with a probable error of $0{\cdot}00000073$. Hence it is evident that the uncertainty attending Bessel's determination of the coefficient of temperature is far greater than its probable error indicates. Upon this ground M. Peters maintains that to ascertain beyond doubt the influence of temperature, a series of direct measures of the focal length of the instrument is indispensable. Until these be executed, every determination of parallax which is deduced in such a way that the uncertainty in the value

of ε is capable of exercising a sensible influence on the result, ought to be regarded as illusory.

The value of ε, which M. Peters deduced from Schlüter's observations, considerably exceeds that which M. Wichmann obtained for the same coefficient by a discussion of his own observations of the sum of the distances, upon the hypothesis that the effects of parallax are insensible. M. Peters therefore maintains that no weight can be attached to the objection urged by M. Wichmann to that hypothesis, founded upon the large magnitude which it assigned to the coefficient of temperature, compared with the value of the same element which Bessel deduced from his researches.

M. Peters also found that the values of ε deducible from experimental measures executed by M. Struve with the Dorpat and Pulkowa refractors, were, in both cases, considerably greater than Bessel's value.

Besides a variation depending on the season of the year, and indicating an erroneous value of Bessel's coefficient of temperature, it appeared that the observations, both of Schlüter and Wichmann, were frequently affected with a constant error for each day of observation. This error appeared to be totally independent of the indications of the thermometer, and was found to affect the measures to the extent of 1″ in twenty-four hours.

M. Peters remarks that the outstanding errors by which the sums of the distances are affected when Bessel's coefficient of temperature is employed, and the difference of the parallaxes of the stars of comparison is supposed to be insensible, are attributable partly to an error in the value of the coefficient of temperature, and partly to unknown causes. He is of opinion, however, that the difference between the temperature of the interior of the telescope and the temperature outside, exercises a considerable influence upon these errors.

With respect to the value of the parallax of A, which M. Wichmann deduced from the differences of the observed distances of the stars of comparison, viz., 0″·135, with the probable error 0″·013, M. Peters considers it to be a result entitled to great confidence. In this case the measured distances of the stars of comparison from the principal star being nearly of equal magnitude, any constant error for each day of observation must necessarily almost disappear from their differences, whether arising from the optical imperfection of the instrument, the unequal heating of the air in the interior of the tube, the difference of the temperature within and the temperature outside, the uncertainty in the value of the coefficient of temperature employed in reducing the observations or the combined operation of all these causes.

Although the parallax which M. Wichmann obtained by a discussion of the differences of the distances was admitted by M. Peters to constitute a good approximation to the true value, still it occurred to him that, on account of the importance of the subject, it would be desirable to deduce the same result by such a method that the uncertainty in the value of the thermometric coeffi-

cient, and especially every error calculated to operate proportionally to the measured distance, would be *totally* eliminated. He, therefore, subjected the observations of Schlüter and Wichmann to a process of treatment, similar to that by which on a former occasion he had deduced the parallax of 61 *Cygni*. In this manner he found the relative parallax of A to be $+0''{\cdot}141$, with the probable error $0''{\cdot}013$, and estimating the parallaxes of the stars of comparison by the aid of his former researches on the parallaxes of the stars of different magnitudes, he found the absolute parallax of A to be $0''{\cdot}148$.

This result agrees very nearly with that which M. Wichmann deduced from the differences of the distances, and appears to M. Peters to afford a sufficient confirmation of his remark, that the latter result is influenced in only a very small degree by the uncertainty in the value of the coefficient of temperature, or any other cause of error which may operate proportionally to the distance.

Memorie dell' Osservatorio dell' Universita Gregoriana del Collegio Romano, diretto dai PP. della Compagnia di Gesu. Anno 1851.

This volume contains a record of the labours of Prof. Secchi at the Observatory of the Collegio Romano during the year 1851. It is divided into two Parts. The first relates to astronomical subjects; the second is devoted to observations of a purely meteorological nature. Part I. is divided into four sections. The first section contains the observations with the meridian circle. The second is devoted to observations of comets and of the newly-discovered planets with the Cauchoix refractor. The third contains a variety of physical observations of *Saturn* and his rings. It appears from these observations, that the interesting phenomenon of a difference of colour at the opposite ansæ of the obscure ring, which was noticed by Mr. Lassell during his late residence at Malta,* had been remarked by Prof. Secchi in the autumn of 1851. The following passage, extracted from the record of the observations of the planet, may be cited in relation to this fact:—

"Sept. 2, 1851. The obscure ring is very distinct, the Cassinian division is well defined, and there are some traces of a subdivision of the exterior ring. The obscure ring is more distinct, and somewhat of a rose colour on the left side of the observer; on the right side it appears of a bluish colour."

The fourth section of Part I. contains an account of the researches of Sig. Sestini on the colours of the stars. There is an Appendix to the work, containing an account of researches upon the distribution of heat on the surface of the sun, and also experiments on the azimuthal motion of the pendulum, instituted with the view of proving the rotation of the earth.

* See p. 148 of vol. xiii. of the *Monthly Notices*.

Theorie der Pendelbewegung mit Rücksicht auf die Gestalt und Bewegung der Erde, von P. A. Hansen, Director der Sternwarte in Gotha. (Neueste Schriften der Naturforschenden Gesellschaft in Danzig, Fünften Bandes Erstes Heft. Danzig, 1853.)

It is stated on the Title-page that this is a Prize Memoir, which was crowned by the Physical Society of Danzig on the 3d of January, 1853. It contains an investigation of the various circumstances which affect the motion of a pendulum at the surface of the earth, whether we suppose the pendulum to be a mathematical point or a physical agglomeration of particles. In a short introduction, M. Hansen has given an account of some of the more important results to which he was conducted by his researches on the subject.

The Memoir is divided into seven sections. In the first section the author investigates the equations of motion of the simple pendulum, taking into account the figure and motion of the earth. Besides its diurnal rotation, the displacement of the earth, whether arising from its annual motion round the sun, or from the motion of the whole solar system in space, is considered in this part of the inquiry. It is found, however, that in both of the last-mentioned cases, the effect produced upon the motion of the pendulum is insensible.

The investigation of the motion of the pendulum is reduced to that of a freely-moving body by representing the tension of the pendulum-rod as an indeterminate force acting in the direction of its length. Eliminating this force from the differential equations of motion of the pendulum, M. Hansen obtains the equations of motion of a body, whether falling freely or projected from a given point in space. Although there was no novelty in the step, he takes occasion to integrate the equations in these two cases, omitting the effects due to the resistance of the atmosphere and the curvature of the earth's surface. The results are somewhat curious. First, in the case of a body falling freely, it is shown that if the height be very great, the deviation towards the *south* will be considerable. For small heights the displacement of the body in that direction will be imperceptible, but on the other hand it will be found to deviate sensibly towards the *east*. Thus, if a body fall from a height of 100 mètres under the equator, the deviation towards the east will be $0^m{\cdot}02199$, and for the same height in latitude $+45°$ the deviation towards the south will be only $0^m{\cdot}000001$, a quantity which is quite insensible.*

M. Hansen next investigates the motion of the body when it is projected in any azimuthal direction whatever, and at any inclina-

* It is said that Oersted was the first who remarked that falling bodies, besides deviating towards the east, deviate also a little towards the south. The fact was confirmed by subsequent experiments, but it remained unexplained until the discussion occasioned by Foucault's experiment led to a knowledge of its true cause, viz. the diurnal rotation of the earth.—EDITOR.

tion with respect to the horizon; and he assigns the formulæ which the expressions for the co-ordinates assume in the two extreme cases of the initial impulse being parallel and perpendicular to the horizon. When the impulse is wholly horizontal, he shows that to an observer considering the body from the origin of motion, it will deviate always from the *left* to the *right* of the plane of projection, no matter what be the direction of the latter in azimuth. When the direction of the impulse is wholly vertical, the body after an assignable time will return to a position in the horizontal plane, from which it was projected, situate a little towards the *west* of its original position. In this case, then, the effect is the reverse of that produced when the body is allowed to fall freely; it is also considerably greater. Thus, if the body attains a maximum altitude of 100 mètres, the deviation towards the west under the equator will be $0^m\,264$, whereas in the case of a body falling freely from the same height, the easterly displacement was found to be only $0^m{\cdot}022$, or barely one-eleventh of the former quantity.

M. Hansen commences the second section by a process of integration which furnishes him with a complete differential equation of the first order, relative to the motion of the simple pendulum, and he then proceeds to investigate the differential equations of motion for the compound or physical pendulum. In this case the rotatory motion of the pendulous mass is referred to a system of rectangular co-ordinates having the origin at the point of suspension, one of these axes being conceived to pass through the centre of gravity of the pendulum. With a view to simplify the problem, M. Hansen supposes the axes of rotation to be *principal axes*, and he further assumes that the moments of inertia about the axes which are perpendicular to the line passing through the centre of gravity and the point of snspension, or third axis of rotation, are equal to each other. This limitation obviously includes among various forms of construction, the pendulum as usually employed in experiments relative to the rotation of the earth, which has generally consisted of a homogeneous spherical ball, suspended from a fixed point by a slender thread or wire.

By integrating the equations of motion, M. Hansen obtains two *complete* differential equations of the first order, and a third equation of a similar kind, the terms of which are *imperfectly* divested of the integral sign. From one of the former of these equations it appears that, independently of any initial rotatory movement, the pendulum in the course of its oscillations will acquire a motion of rotation about the axis passing through the centre of gravity and the point of suspension. It is shown further that this rotatory motion is not of an oscillatory nature, but has a tendency to go on continually in the same direction.

In the third section, M. Hansen obtains a first approximation to the complete integrals of the equations of motion of the pendulum. First, considering the times during which the successive oscillations are performed, he shows that they are modified not only by the diurnal rotation of the earth, but also *by the rotation*

of the pendulum about its own axis. The correction, however, which is due to each of these causes, is so small in all actual experiments with the pendulum as to be quite insensible. Supposing the pendulum to consist of a very small homogeneous spherical body, suspended by a thread or slender metallic wire, and to perform one of its oscillations in a second of time, the correction depending on the rotation of the earth would be less than the three thousand millionth part of a second. Even if the pendulum extended to the enormous length of 900 mètres, the correction to the time of oscillation—which would in this case amount to 30 seconds —would be less than the hundred thousandth part of a second. Similar results are obtained with respect to the correction depending on the rotation of the pendulum about its own axis, supposing the diameter of the pendulum relative to its length to be small, and the velocity of rotation to be inconsiderable. Thus, supposing the diameter of the pendulum to be one-fortieth of its length, as in the case of the original experiment of Foucault, M. Hansen found that for a correction amounting to the twenty thousandth part of a single oscillation, the pendulum would require to make forty complete revolutions upon its axis during the time of each oscillation; a velocity of rotation so considerable that there would be no difficulty in guarding against it.

M. Hansen proceeds next to consider the path described by the pendulum in space. Confining his investigation to terms of the second order, he finds that the plane of oscillation is subject to a continual variation in azimuth depending on the diurnal rotation of the earth, the direction of azimuthal motion being always from east to west through south, or contrary to the diurnal motion of the earth, and the velocity of revolution being equal to the angular velocity of the earth around its axis multiplied by the latitude of the place of observation.

It appears further, from the researches of M. Hansen, that a *rotatory movement of the pendulum about its own axis causes the plane of oscillation to vary continually in azimuth.* The effect produced in this case will tend to diminish or to increase the azimuthal variation depending on the diurnal rotation of the earth, according as the pendulum rotates upon its axis in the direction of the earth's diurnal rotation, or in the opposite direction.

The pendulum is constrained, by the circumstances above mentioned, to pursue a path which does not anywhere intersect the vertical line, passing through the point of suspension. M. Hansen describes its movement by stating it to revolve upon the surface of an elliptical cone, the axes of the base of which continually vary in position with respect to the meridian of the observer, and he assigns the conditions which determine the magnitude and position of the axes of the ellipse.

M. Hansen found, that by no means a very considerable rotation of the pendulum about its axis would be necessary to produce an azimuthal variation of the plane of oscillation equal to or even exceeding the effect depending on the diurnal rotation of the earth,

Hence it is manifest, that from this cause the plane of oscillation might revolve in a direction actually opposite to that in which it would otherwise revolve. The amount of azimuthal motion depends in this case, not on the absolute length of the pendulum, but on the proportion of the diameter to the length; and, *cæteris paribus*, it gradually becomes less as the diameter of the pendulum is diminished. M. Hansen accordingly recommends, that in all experiments of this kind, the pendulum should be constructed of some substance having a high specific gravity, such as lead; and he suggests that care should be especially taken to guard against a rotatory motion of the pendulum upon its axis.

M. Hansen next considers the case in which the pendulum revolves upon the surface of a cone with a circular basis. Supposing the rotation upon its axis to vanish, he finds that the time which the pendulum will take to accomplish a complete revolution upon the surface of the cone will be different, according as the pendulum revolves from east to west through south, or in the opposite direction. For any place of observation in the northern hemisphere the time of revolution will be less in the former case than it will be in the latter; for any place in the southern hemisphere the result will be the reverse of this.

The difference between the times of a complete revolution of the pendulum, according as it revolves in one or the other of the directions just mentioned, is found to be independent of the initial elongation of the pendulum from the vertical direction; it is also found to change very slowly when the basis of the cone differs only in a slight degree from a circle. Supposing the length of the pendulum to be 10 mètres and the latitude to be 51°, M. Hansen found that, in the case of a pendulum revolving upon the surface of a cone, the base of which is perfectly circular, the difference between the times of a complete revolution, according as it moves in one direction or in the other, amounts to $0^s{\cdot}00073$. This is, indeed, a very small quantity. It may, however, be rendered perceptible by the accumulated effect of many revolutions, as the experiments of M. Bravais have shown.

M. Hansen finally deduces the following theorem from his first approximation to the integrals of the equations of motion of the pendulum:—"*If, at the beginning of the motion, a plane having a fixed position with respect to the mass of the pendulum be conceived to pass through the axis of the latter, and also through the vertical line* [*in which the point of suspension is situate*], *this plane will continue to pass through the vertical line at each successive maximum elongation of the pendulum, notwithstanding the azimuthal motion of the plane of oscillation; provided that, at the beginning of motion, no impulse communicating a rotatory motion about its axis be applied to the pendulous body.*" The plane referred to will, in fact, always pass through the vertical line, abstracting from consideration a small periodical deviation. "Thus," says the author, "in like manner, as the moon—although from a different cause—always turns the same side towards the earth, with

the exception of the slight fluctuations due to the effect of libration, so does the pendulum exhibit the same surface to an eye situate in the vertical, and viewing it in any direction whatever."*

In the fourth section M. Hansen investigates the effects deducible from further approximations to the integrals of the equations of motion. In this part of his researches he employs the theory of the variation of arbitrary constants.† From the second approximation he derives a term expressing an azimuthal motion of the plane of oscillation, and depending, for any given length of the pendulum, on the product of the maximum and minimum elongations from the vertical. If no lateral impulse be communicated to the pendulum at the commencement of the oscillations, the azimuthal motion of the plane of oscillation (or, which amounts to the same thing, the azimuthal motion of the axes of the ellipse), as already determined by the first approximation, will not be sensibly affected by the additional term. When this condition, however, is not satisfied, the result will be different, but it may be found by the aid of the following consideration. Since any lateral impulse directly affects the semi-minor axis of the ellipse, we are enabled, from observations of the magnitude of the latter in any experiment, to infer, *à posteriori*, the intensity of the lateral force, and also the quantity of the azimuthal motion of the plane of oscillation depending on its influence. Supposing the length of the pendulum to be 10^m, and the greatest and least elongations from the vertical to be $\frac{1}{9}$ and $\frac{1}{400}$‡ respectively, M. Hansen finds that the additional quantity of azimuthal motion resulting from the second approximation would then amount, for any place of observation in Central Europe, to one-fourth of the effect depending on the rotation of the earth, as already determined by the first approximation. It is manifest, therefore, that a much smaller value of the minimum elongation than that above-mentioned would suffice to exercise a sensible influence on the motion of the plane of oscillation.§

The term above referred to is the only one among those fur-

* From the terms in which M. Hansen alludes to this curious principle (*ich bekomme ferner einen neuen Satz*), he appears to think that it had been hitherto unknown. It is right to mention, however, that soon after the announcement of Foucault's experiment, the theorem in question was pointed out to be a necessary consequence of the rotation of the earth; and it was also said to have been fully verified by subsequent experiments with the pendulum. (See for example, *Phil. Mag.*, June 1851, also *Mec. Mag.*, vol. liv., p. 326.) It is not improbable, however, that M. Hansen may have been the first who demonstrated the theorem by an analytical process.

† In the *Philosophical Magazine* for October 1851, there is a paper by the Rev. J. A. Combe, "*On the Motion of the Apse Line in the Pendulum Oval*," containing an elegant application of the theory of the variation of arbitrary constants. The rotation of the earth, however, is not taken into consideration.

‡ These fractions are supposed to refer, not to the mètre, but to the length of the pendulum as the standard unit.

§ For an investigation of the effect produced on the motion of the axes of the ellipse (without reference to the rotation of the earth), when the oscillations are nearly circular, and when they are almost in the same plane, see two papers by Mr. Airy, in vols. xi. and xx. of the *Mem. R. Ast. Soc.*

nished by the second approximation which M. Hansen found to be capable of producing a sensible effect.

In the fifth section M. Hansen considers the effect of the resistance of the atmosphere on the motion of the pendulum. The investigation is conducted upon two distinct hypotheses respecting the law of resistance, namely, when it is proportional to the simple power of the velocity, and when it is proportional to the square of the velocity. The effect is also considered when the oscillations are nearly in the same plane, and when they are nearly circular. The results deduced from the two hypotheses are essentially different, insomuch that observations instituted for the purpose, might not improbably indicate the hypothesis which most nearly represents the actual effect.

The sixth section of M. Hansen's Memoir is devoted to an investigation of the effect of the torsion of the thread or wire by which the pendulum is suspended.

In the seventh section M. Hansen proposes a new form of the pendulum, designed to obviate certain disadvantages attending the usual mode of construction. He remarks, that if the pendulum be suspended in the ordinary way, as described by him in the preceding part of his Memoir, it is indispensable, in order to avoid other sources of disturbance than those which he has investigated, that the thread or wire by which the pendulum is suspended should be homogeneous in its structure, and that its transverse section should be everywhere exactly circular. Especial care should be taken that the thread, in consequence of the pressure at its upper end, should not lose its circular-cylindrical form, since, from this cause alone, the variation of the plane of oscillation would be different in different azimuths. The stiffness of the thread also exercises an influence upon the law of the diminution of the arcs of oscillation which would otherwise result from the resistance of the atmosphere. M. Hansen, accordingly, is of opinion, that the following construction of the pendulum, inasmuch as it is free from those disturbing influences, is preferable to the usual form.

Instead of a slender wire, let the pendulum be suspended by a metal rod, so thick as to preclude all possibility of torsion. Let the ball be attached to the lower end of the rod, so that, if the axis of the latter were prolonged, it would pass through the centre of gravity of the ball. Let the rod be connected at the upper end with an apparatus of metal, terminating underneath in a spherical segment of small radius, the centre of which also lies in the prolongation of the axis of the pendulum rod. This spherical segment, composed of hardened steel or hard stone, being placed upon a horizontal plane surface of the same material, will constitute the point of suspension, as in earlier apparatuses the pendulum was made to swing by being suspended upon a knife-edge resting on a horizontal plane. It is intended that, during the oscillations of the pendulum, the supporting spherical segment shall *roll*, but not *slide*. For small arcs—of which only there is here any question—this object will be attained by merely smoothing the surfaces of the

spherical segment and its supporting plane without communicating to them a high polish.

M. Hansen remarks, that the form of construction here proposed by him especially possesses the advantage that, when the oscillations lie in one plane, or nearly so, a rotation of the pendulum about its axis (which he had shown to arise readily from the azimuthal motion of the plane of oscillation depending on the diurnal rotation of the earth) is no longer possible. The remainder of the section is occupied with an investigation of the motion of such a pendulum.

On the Secular Variation of the Moon's Mean Motion.

By J. C. Adams, Esq., M.A. F.R.S. &c.*

The author remarks, that in treating a great problem of approximation, such as that presented to us by the investigation of the moon's motion, experience shows that nothing is more easy than to neglect, on account of their apparent insignificance, considerations which ultimately prove to be of the greatest importance. One instance of this occurs with reference to the secular acceleration of the moon's mean motion. Although this acceleration and the diminution of the eccentricity of the earth's orbit, on which it depends, had been made known by observation as separate facts, yet many of the first geometers altogether failed to trace any connexion between them, and it was not until he had made repeated attempts to explain the phenomenon by other means, that Laplace himself succeeded in referring it to its true cause.

The accurate determination of the amount of the acceleration is a matter of very great importance. The effect on the moon's place, of an error in any of the periodic inequalities, is always confined within certain limits, and takes place alternately in opposite directions within very moderate intervals of time, whereas the effect of an error in the acceleration goes on increasing for an almost indefinite period, so as to render it impossible to connect observations made at very distant times.

In the *Mécanique Céleste*, the approximation to the value of the acceleration is confined to the principal term, but in the theories of Damoiseau and Plana, the developements are carried to an immense extent, particularly in the latter, where the multiplier of the change in the square of the eccentricity of the earth's orbit, which occurs in the expression of the secular acceleration, is given to terms of the seventh order.

As these theories agree in principle, and only differ slightly in the numerical value which they assign to the acceleration, and as they passed under the examination of Laplace, with especial reference to this subject, it might be supposed that only some small numerical rectifications would be required in order to obtain a very exact determination of this value.

It has not been, therefore, without surprise, which he has no doubt will be shared by the Society, that the author has lately

* Proc. of the Roy. Soc. June 16, 1853.

found that Laplace's explanation of the phenomenon in question is essentially incomplete, and that the numerical results of Damoiseau's and Plana's theories, with reference to it, consequently require to be very sensibly altered.

Laplace's explanation may be briefly stated as follows. He shows that the mean central disturbing force of the sun, by which the moon's gravity towards the earth is diminished, depends not only on the sun's mean distance, but also on the eccentricity of the earth's orbit. Now this eccentricity is at present (and for many ages has been) diminishing, while the mean distance remains unaltered. In consequence of this, the mean disturbing force is also diminishing, and therefore the moon's gravity towards the earth at a given distance is, on the whole, increasing. Also the area described in a given time by the moon about the earth is not affected by this alteration of the central force; whence it readily follows that the moon's mean distance from the earth will be diminished in the same ratio as the force at a given distance is increased, and the mean angular motion will be increased in double the same ratio.

This, the author states, is the main principle of Laplace's analytical method, in which he is followed by Damoiseau and Plana; but it will be observed that this reasoning supposes that the area described by the moon in a given time is not permanently altered, or, in other words, that the tangential disturbing force produces no permanent effect. On examination, however, he remarks it will be found that this is not strictly true, and he proceeds briefly to point out the manner in which the inequalities of the moon's motion are modified by a gradual change of the disturbing force, so as to give rise to such an alteration of the areal velocity.

As an example, he takes the case of the *variation*, the most direct effect of the disturbing force. In the ordinary theory, the orbit of the moon, as affected by this inequality only, would be symmetrical with respect to the line of conjunction with the sun, and the areal velocity generated while the moon was moving from quadrature to syzygy, would be exactly destroyed while it was moving from syzygy to quadrature, so that no permanent alteration would be produced.

In reality, however, the magnitude of the disturbing force by which this inequality is caused, depends in some degree on the eccentricity of the earth's orbit; and as this is continually diminishing, the disturbing forces at equal intervals before and after conjunction will not be exactly equal. Hence the orbit will no longer be symmetrically situated with respect to the line of conjunction, and therefore the effects of the tangential force before and after conjunction no longer exactly balance each other.

The other inequalities of the moon's motion will be similarly modified, especially those which depend, more directly, on the eccentricity of the earth's orbit, so that each of them will give rise to an uncompensated change of the areal velocity, and all of these must be combined in order to ascertain the total effect.

Since the distortion of the orbit just pointed out is due to the

change of the disturbing force consequent upon a change in the eccentricity of the earth's orbit, and the action of the tangential force permanently to change the rate of description of areas, is only brought into play by means of this distortion, it follows that the alteration of the areal velocity will be of the order of the square of the disturbing force multiplied by the rate of change of the square of the eccentricity. It is evident that this alteration of areal velocity will have a direct effect in changing the acceleration of the moon's mean motion.

Having thus briefly indicated the way in which the effect now treated of originates, the author proceeds with the analytical investigation of its amount. In the present communication, however, he proposes to confine his attention to the principal term of the change thus produced in the acceleration of the moon's mean motion, deferring to another, though he hopes not a distant opportunity, the fuller treatment of this subject, as well as the determination of the secular variations of the other elements of the moon's motion, which, arising from the same cause, have also been hitherto overlooked.

In the usual theory, the reciprocal of the moon's radius vector is expressed by means of a series of *cosines* of angles formed by combinations of multiples of the mean angular distance of the moon from the sun, of the mean anomalies of the moon and sun, and of the moon's mean distance from the node; and the moon's longitude is expressed by means of a series of *sines* of the same angles, the coefficients of the periodic terms being functions of the ratio of the sun's mean motion to that of the moon, of the eccentricities of the two orbits and of their mutual inclination.

Now, if the eccentricity of the earth's orbit be supposed to remain constant, this is the true form of the expressions for the moon's co-ordinates; but if that eccentricity be variable, the author shows that the differential equations cannot be satisfied without adding to the expression for the reciprocal of the radius vector, a series of small supplementary terms depending on the *sines* of the angles whose *cosines* are already involved in it, and to the expression for the longitude, a series of similar terms depending on the *cosines* of the same angles; all the coefficients of these new terms containing as a factor the differential coefficient of the eccentricity of the earth's orbit taken with respect to the time.

The author first determines as many of these terms as are necessary in the order of approximation to which he restricts himself, and then takes them into account in the investigation of the secular acceleration. The expression which he thus obtains for the first two terms of this acceleration, is,

$$-\left(\tfrac{3}{2}m^2-\frac{3771}{64}m^4\right)\int(e'^2-\mathrm{E}'^2)\,n\,dt.$$

According to Plana, the corresponding expression is

$$-\left(\tfrac{3}{2}m^2-\frac{2187}{128}m^4\right)\int(e'^2-\mathrm{E}'^2)\,n\,dt.$$

It will be observed that the coefficient of the second term has been

completely altered in consequence of the introduction of the new terms.

The numerical effect of this alteration is to diminish by 1″·66 the coefficient of the square of the time in the expression for the secular acceleration; the time being, as usual, expressed in centuries.

It will, of course, be necessary to carry the approximation much further, in order to obtain such a value of this coefficient as may be employed with confidence in the calculation of ancient eclipses.

Considerations of a General Nature relative to the small Planets situate between Mars and Jupiter. By M. Le Verrier.*

M. Le Verrier commences this brief communication by remarking that the hypothesis of Olbers respecting the origin of the small planets, which the great inclination of the orbit of *Pallas* did not serve to confirm, has been completely negatived by the numerous discoveries of recent years. It is to be hoped, he adds, that, since the hypothesis of Olbers must be renounced, the knowledge of a great number of such bodies may finally lead to the discovery of some law regulating their distribution in space. He then proceeds to some general considerations respecting the twenty-six planets already discovered, omitting all reference to the twenty-seventh planet which had been recently discovered by Mr. Hind.

The small planets revolve in a zone which commences at the mean distance of 2·20 from the sun, and extends to the distance 3·16. The mean value of their excentricities is 0·155. The magnitude of the excentricity in individual cases does not seem to have any relation either to the mean distance from the sun or to the position of the perihelion.

The inclinations of the orbits, whether with respect to each other or to the ecliptic, are in both cases considerable. The individual magnitude of the inclinations does not appear to depend either on the mean distance from the sun or on the direction of the ascending node.

The perihelia and the ascending nodes offer some peculiar features. *Twenty* of the perihelia have their longitudes included between 4° and 184°. *Twenty-two* of the ascending nodes have their longitudes between 36° and 216°. M. Le Verrier appears somewhat inclined to suspect the existence of a systematic difference between the mean direction of the ascending nodes of the planets nearer the sun and that of the ascending node of the planets that are more remote, indicating the existence of two distinct groups. He then proceeds to investigate the superior limit of the total quantity of matter which may be distributed in the zone of the heavens under consideration.

An inquiry of this nature can only be conducted by an exa-

* *Comptes Rendus*, Nov. 28, 1853.

mination of the action of such a mass of matter on the neighbouring planets *Mars* and the Earth. It is manifest that the periodic inequalities resulting from the action of the small planets (whose aggregate mass it is proposed to determine) cannot fail mainly to destroy each other by their opposite effects at any assigned instant, and, therefore, must form an unsuitable groundwork for such an investigation.

The secular inequalities being independent of the relative positions of the planets, are not attended with the inconvenience to which reference has just been made, M. Le Verrier remarks that the secular variation of the perihelion of the Earth or *Mars* contains two terms depending on the action of the small planets, one of which is especially adapted for determining the aggregate of their mass. This term depends exclusively on the mean distance of the disturbing planet from the sun, and on the excentricity of the disturbed planet; further, it is essentially positive. It is manifest, therefore, that if the zone, which includes the small planets, contains an innumerable number of such bodies, their respective masses will act together upon the perihelion of the disturbed planet in the same manner as if they were concentrated into one single mass acting at a suitable distance.

With respect to the second term, which is of the same order of magnitude with the first, M. Le Verrier remarks that it depends on the excentricity and the longitude of the perihelion of the disturbing body. In the case of the Earth, since its perihelion is placed almost in the middle of the region of the heavens occupied by the perihelia of more than three-fourths of the small planets, this term might become sensible and contrary in sign to the first term, more especially as the mean excentricity of the small planets is nine times as great as the excentricity of the terrestrial orbit. M. Le Verrier, upon this ground, rejects the secular variation of the terrestrial perihelion as incapable of furnishing a definitive solution of the question under consideration, although its actual value has been ascertained with greater accuracy than that of *Mars*.

With respect to the perihelion of *Mars*, it is more favourably situated relatively to the perihelia of the small planets; moreover, the excentricity of this planet is considerable. By reason of both these circumstances, it happens in this case that the second term does not amount to more than one-fourth of the first term, and it may be expected that this superiority of the first term will continue to subsist after numerous future discoveries of small planets.

Agreeably to these remarks, M. Le Verrier finds that if the aggregate of the masses of the small planets equalled the Earth's mass, it would occasion a variation of the heliocentric longitude of the perihelion of *Mars* amounting to eleven seconds of space in a century. Now, if a variation amounting to even the fourth of this quantity affected the perihelion of the planet, it could not fail to be detected by astronomers, and as no evidence of such a variation exists, it follows as a necessary consequence, *that the total quantity of matter composing the small planets situate between the*

mean distances 2·20 and 3·16 cannot amount to more than about a quarter of the Earth's mass.

M. Le Verrier remarks that a similar result might be obtained by a consideration of the secular variation of the plane of the ecliptic, but in this case the limit would not be so confined.

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ROYAL ASTRONOMICAL SOCIETY.

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G. B. AIRY, Esq., in the Chair.

J. R. Christie, Esq., Royal Military Academy, Woolwich;
Rev. W. J. Read, Huddersfield Collegiate School;
Eyre Burton Powell, Esq., Madras Native College;
James William Grant, Esq., Wester Elchies, Morayshire; and
Rev. Robert Harley, Airedale College, Bradford,

were balloted for and duly elected Fellows of the Society.

The Editor has been requested to announce to the Fellows of the Society that there still remains the sum of seventeen pounds to be realised in order to complete the amount required for defraying the expense of the bust of Mr. Adams, which it has been proposed to place in one of the apartments of the Society. Those who have not yet contributed towards this object are consequently reminded that it is not yet too late to co-operate in the matter, while such as have already forwarded the limited sum required of them, according to the terms of the original announcement of the project, will, no doubt, be glad to have the opportunity of adding a little to their former subscriptions. As it is desirable to bring the engagement with the artist to a definitive conclusion as speedily as possible, it is to be hoped that those Fellows of the Society who are favourable to the project will lose no time in using a little exertion among their friends towards procuring their aid in raising the necessary sum. Subscriptions will be received by the Rev. T. Pelham Dale, at the Apartments of the Society, Somerset House, or at his residence, Sion College, London Wall.

Observations of the Second Comet of Klinkerfues (Comet V. 1853) made with the Equatoreal at Hamburg. By M. C. Rümker.*

1853.	Hamb. M.T. h m s	Comet's R.A. ° ′ ″	Comet's Decl. ° ′ ″	Com.
Dec. 22	6 15 27·9	16 57 41·9	+28 46 49·7	8
25	7 0 31·0	37 46·7	25 52 57·3	15
28	7 18 45·0	16 25 51·1	+23 13 0·9	8

* It appears that this comet, which was first discovered in Europe, at Göttingen, on the 2d of December, 1853, had been already discovered at Newark, in the State of New York, U.S., on the 25th of November.

Apparent Positions of Compared Stars, according to Re-observation with the Meridian Circle.

1858.	App. R.A.	App. Decl.
	h m s	° ′ ″
Dec. 22	1 5 57·267	+28 47 57·7
25	5 35·878	25 40 44·7
...	6 49·382	25 40 25·8
...	4 51·485	22 41 16·3
...	8 57·531	23 26 7·7
...	1 10 15·788	+23 6 45·5

Observations of Euterpe with the Meridian Circle.
By M. C. Rümker.

1858.	Hamb. M.T.	R.A.	Decl.	Log $\frac{q}{P}$
	h m s	° ′ ″	° ′ ″	
Dec. 10	9 34 11·7	43 2 23·3	+14 51 17·9	9·7969
13	9 21 10·4	42 43 57·4	14 50 33·9	9·7962
14	9 16 54·4	42 38 54·7	14 50 40·0	9·7966
22	8 44 7·0	42 18 49·9	14 57 5·5	9·7951
25	8 32 26·4	42 20 37·4	15 2 18·1	9·7942
28	8 21 6·9	42 27 41·5	+15 8 54·4	9·7932

Some Suggestions in Explanation of the Cause of the Primitive Incandescent Condition of the Earth and other Planets. By James Nasmyth, Esq.

Among the many facts which geological research has developed there appears none more clearly established than that of the igneous, or molten, condition of the entire mass of our globe at some vastly remote era of its physical history; and that its present condition is the result of a succession of changes consequent on the escape, or passage into space, of the greater portion of that primitive heat, the residue of which yet manifests itself deep under the now solid crust of the earth in those molten outbursts which the now comparatively few active volcanoes vomit forth, and which we may consider to be the expiring vestiges of the once universal molten state of our globe.

As I have not met with any attempt to trace to its source, or assign a cause of, this primitive molten condition of the earth, in the most earnest, but humble spirit of philosophic suggestion and inquiry, I desire to offer in this brief form the result of some thoughts on this interesting subject, in the hope that the following remarks may chance to suggest further investigation, and so yield results more worthy of so grand a subject.

In order to state in the most simple form *the principle* upon which I base my speculations on the source of the primitive heat of our globe, I would refer to the well-known principle in the laws of matter,—namely, that when matter, whether in the solid, fluid, or

gaseous condition, is, by some external or internal force or agency, caused to occupy less space, heat is evolved.

Applying this general principle to what there is such strong reason to suppose was the first condition of that matter which was destined to form our globe, and carrying our ideas back to the first moments of its physical history at which we may suppose it to have been summoned forth into existence as a nebulous mass, either distinct and separate, or as a separated portion from a greater nebulous mass, and granting that the law of gravitation was co-existent,—it appears to me that if we assume these conditions, the inevitable result of the action of the law of gravitation, operating on the particles of matter composing a nebulous mass, would be a progressive decrease, or collapse, of the original volume of such nebulous mass; and that the result of this decrease of volume by the collapse action of gravitation would be accompanied rise of temperature, more especially at the centre of gravity of the mass, where a nucleus would be formed, and upon whose surface myriads of particles would come crowding inwards and attach themselves. While, by the general collapse of the entire mass of the nebulous body, resulting, as before said, from the action of gravitation of its particles towards its centre of gravity, that heat which was latent in the original or primitive expanded volume of the nebulous mass would come forth and manifest itself as active heat, most intense nearest to where the focus of action lay, where it would result in a glow of fervent intensity, of which we can form no adequate conception.

In this manner I conceive the temperature of the nucleus would continue to increase, while the dimensions or volume of the nebulous mass went on diminishing, through ages of time, until the temperature of the nucleus reached such a pitch of intensity, as to begin to check the accelerating influx of particles by the dispersive influence of the intense heat of the nucleus. Then would ensue an era of retardation in the progressive accumulation of matter upon the nucleus; and its after history would most probably be governed by the combined action of gravitative accumulation and those changes which would result from the continual escape of the heat of the remaining nebulous envelope, and so render the matter of which it was composed more subject to the attractive influence of the globe now existing within it.

I conceive that countless ages might thus elapse, through the mutual action of the agencies I have referred to, ere such a globe had commenced the earliest stages of its geological history, which would date from that period when all further accession of temperature was at an end, and the nucleus (now a planet) began to part with its primitive heat by its radiation into space.

Thus I have endeavoured to assign as the cause of the primitive incandescent temperature of planetary masses, the action of gravitation upon the nebulous matter, of which they are conceived to have been formed: the action of gravitation overtaking in its collapsing influence that gradual decrease of volume, which might otherwise have occurred through simple contraction, and so

expressing the heat latent in the nebulous volume, and causing it to come forth as sensible heat in most active condition, and so manifest itself in a state of intense incandescence in the nucleus or planetary mass.

Should these remarks tend in the most humble degree to suggest some further thoughts on this subject to such of the members of the Royal Astronomical Society who may happen to turn their attention to these speculations, I shall feel glad in having thus ventured on suggesting them.

Note on the Relations of the Perihelia and Nodes of Cometary Orbits. By Edward Cooper, Esq.

"In the Introduction to my little work on Cometic Orbits, published last year, I called the attention of astronomers to the somewhat remarkable result of an investigation into the places of the perihelia and nodes of periodic comets, and of a comparison with those of the then known planets.

"The number of comets then known, or supposed to complete their revolutions in times under 500 years, was 23; and it so happened that the then known number of planets was also 23. Since the date of the Introduction I have mentioned twelve new asteroids have been added to the preceding 23; thus producing a total of 35 planets discovered up to the present date. It appeared to me to be desirable to class these bodies, as I had previously done the former; and the result at present stands thus:—

"*L. P.'s of Planets.*

Hel. L.

Between	45°	and 135°	.....	13	
	135	— 225	 5	11	24
	225	— 315	 6		
	315	— 45	....	11	

"The numbers formerly were 16 and 7; now we have 24 and 11: a fact which appears materially to strengthen the suspicion of some yet undiscovered law. However, when the same process is applied to the nodes, although the result be similar, yet a much more remarkable one arises from the adoption of a different semi-circle.

"*Ω of Planets.*

Hel. L.

Between	45°	and 135°	.....	19	
	135	— 225	 9	13	21
	225	— 315	 4		
	315	— 45	.....	2	

"But if, instead of the semi-circles 315° to 135° and 135° to 315°, we adopt those from 45° to 225° and 225° to 45°, we see

that, of the ascending nodes of 34 planets, 28 are found in the first semi-circle and only 6 in the second. Again, the semi-circles that contain the greatest number of L. P.'s of planets are between 0° and 180° or 10° and 190°. That which contains the greatest number of nodes is between 35° and 215°. In the first case there are 26, and in the latter 29. The quadrant containing the largest number of L. P.'s of planets is that between 11° and 101°, of which there are 16. That containing the largest of nodes is from 35°½ to 125°½, of which there are 20. Perhaps what I have now brought under your notice may interest the Royal Astronomical Society."

A Catalogue of the Right Ascensions of 1009 *Stars contained in the Catalogue of the British Association for the Advancement of Science, being chiefly those of the 6th and 7th magnitudes.* By Lord Wrottesley.

"In the commencement of the year 1842, I resolved on erecting an Observatory near to my residence in Staffordshire; and on the 29th March in that year the first stone of the building was laid by my youngest son, on an elevated position, about 500 yards to the N. by W. of the mansion. On the 11th May following, the transit piers were fixed, and on the 8th of October in the same year, the 5-foot transit, with which I observed my Blackheath Catalogue of 1318 stars, was finally placed upon them, and ready for work in its new locality.

"In the preceding January I had purchased from Mr. Beaumont of Finningley, in the county of York, an achromatic telescope of 10 feet 9 inches focal length, with an object-glass of 7¼ inches clear aperture; of this the flint glass is by Guinand, and the crown by Dollond, who gave the curves to the glasses and completed the instrument. The telescope is equatoreally mounted after the English fashion.

"The building was, therefore, designed to contain the transit and equatoreal above described, with apartments for the residence of an observer.*

"The shaded portion of the accompanying ground-plan, which is surrounded by a 9-inch wall, represents a mass of concrete, which is continuous beneath the flooring of both the transit and equatoreal rooms, and supports the transit and equatoreal piers, and the piers of two sidereal clocks and the driving clock. The wall which divides the equatoreal from the transit-room is built on a brick arch which bestrides the concrete mass, so that the latter, and the piers supported by it, are completely independent of the shell of the building itself. All the piers repose on vast masses of sandstone, which rest on the concrete base.

* The author here subjoins a ground-plan of the Observatory which, he remarks, will save much tediousness of description. He also presents to the Society a view of the south front of the building.

"The wall of the building is double, the outer one is of stone, and is separated by a vacant space of an inch from the interior wall, which is of brick.

"The concrete mass, which is about $4\frac{1}{2}$ feet high, has a paved floor on its north side, and there are gratings at the base of the walls, opening into a drain, which runs round the foundations of the building, and keeps them always dry. The basement story of the building is partly occupied by the kitchen, from which the smoke is conveyed underground to a chimney at a distance, and partly by the concrete mass above described, and which is beneath the flooring of the transit and equatoreal rooms.

"The transit piers, and indeed the whole furniture of the transit-room, the clock included, are, with trifling exceptions, the same as at my former observatory, and the room is of the same identical dimensions. There is, however, an improvement in the machinery employed for opening the roof, which is concealed in the north wall; the plan of it was borrowed from Mr. Beaumont.

"The equatoreal piers and the revolving dome were all purchased with the telescope, and conveyed hither from Yorkshire. The dome having been cut into six parts, was put together again; and the whole building was erected under my superintendence by my own workpeople.

"The three rooms are warmed by a hot-water apparatus, of which the boiler is placed in the kitchen, adjoining the fireplace, which is in the west wall.

"The telescope is driven by clockwork, and in the equatoreal room there is a sidereal clock by Baird, with a mercurial compensation pendulum by Dent.

"The computing-room is furnished with a pier, which rests on a base distinct from the concrete mass. This is intended for the occasional support of a meridian circle, or portable transit. It commands the transit of *Polaris* to the north, and of such stars to the south as have south declination. On it was placed the Lee circle, which the Society were so kind as to lend me for a time.

"There is a detached pier to the south of the building, which carries the transit meridian mark, viewed through a lens, and serves also for the occasional support of a portable transit for observing on the prime vertical, or small telescope.

"The Observatory is furnished with an anemometer by Osler (the vane and pressure-plate of which are represented in the painting), and with barometers and other meteorological instruments.

"The site of the building is, unfortunately, not a very favourable one for astronomical observations; for it is situated between the coal-fields of Staffordshire, which are about six miles to the E.S.E., and those of Shropshire about ten miles to the W.N.W.; there is also a grove of trees to the south. Owing to the first of these causes, and perhaps in some degree owing to the last also, the difficulty is very great of obtaining satisfactory observations of stars having considerable south declination; and not only has the

completion of the Catalogue been much retarded by this cause, but stars have possibly been lost which would otherwise have been obtained.

"The latitude of the Observatory was determined,—first, by the Lee circle, placed on the pier in the computing-room above described; secondly, by observations on the prime vertical, made by the collimating transit of 20 inches focal length, fixed on the detached pier 128 feet to the south of the building; thirdly, by a Troughton's sextant, also placed on the last-mentioned pier."

Lord Wrottesley, for reasons which he assigns, did not deem it advisable to use the observations on the prime vertical in determining the definitive value of the latitude. The observations with the Lee circle, amounting to 101 in number, were made by himself, Mr. Goddard, and Mr. Philpott, between May 1843 and January 1845. They make the latitude $=52^\circ\ 37'\ 3''{\cdot}81$, giving weight to each night's result according to the number of observations on which it depends. The sextant observations give 4 results only, but they are derived from 47 altitudes; they were made by Mr. Hartnup, of the Liverpool Observatory, in April and June 1847. They are very accordant. They give the latitude $=52^\circ\ 37'\ 0''{\cdot}77$, after applying the correction for the pier.

The latitude given by the circle has a probable error of $0''{\cdot}31$, and that derived from the sextant observations of $0''{\cdot}32$. Lord Wrottesley, therefore, took the simple mean, which gave the latitude $=52^\circ\ 37'\ 2''{\cdot}3$, a result which, he remarks, is probably not very far from the truth.

"The longitude," continues Lord Wrottesley, "was also obtained by the kind exertions of Mr. Hartnup, who, though much occupied with the public duties of the office to which he has been so deservedly appointed, and which he fills so ably, has always shown a most praiseworthy zeal and promptitude when I have been compelled to solicit his aid. Mr. Hartnup made two trips with fourteen chronometers, on 6th July and 31st of Aug., 1845, between my Observatory and that of Liverpool, and he himself observed all the transits for obtaining the clock errors at both places, observing on each occasion several stars with each transit instrument, and with the illuminated ends both east and west."

The author here gives the partial values, and the results, which are highly satisfactory. The mean of the first series gives the longitude east of Liverpool Observatory $=3^m\ 6^s{\cdot}5343$; the mean of the second series gives the longitude $=3^m\ 6^s{\cdot}5529$. Consequently, the mean of the two determinations of the longitude East of Liverpool Observatory $=3^m\ 6^s{\cdot}54$. Now the longitude of Liverpool Observatory as finally corrected $=12^m\ 0^s{\cdot}11$. Hence the longitude of Wrottesley Observatory West of Greenwich $=8^m\ 53^s{\cdot}57$, with a probable error of $0^s{\cdot}04$ only.

"The years which immediately succeeded the foundation of the Observatory, were thus employed in obtaining its position, and in observations with the equatoreal, which I had the honour of communicating to the Royal Society, and which are printed in the

Philosophical Transactions for 1851, and in some other observations of double stars with the same instrument, which I intend to lay before the Astronomical Society at some future period.

"When the Catalogue of the British Association made its appearance, I was anxious to assist in correcting the places there given; in short, to perform the same office in respect to that most valuable publication, which I had already undertaken in reference to the prior Catalogue of the Astronomical Society. Also, twenty years having elapsed from the epoch of my former Catalogue, I thought that sufficient time had been allowed for exhibiting the effects of proper motion, and I was curious to see, whether some of the stars which had shown symptoms of it in 1830–5 still continued to manifest the same wandering disposition.

"I selected, therefore, above 1000 stars from the Catalogue of the British Association, with the intention of obtaining at least five observations of each, being chiefly those stars that had been already observed at Blackheath, and had been discovered to possess proper motion, also stars, the right ascensions of which, as given in my Catalogue, differed from those given by other observers; also stars contained in that Catalogue, the right ascensions of which seemed to have been imperfectly determined by other observers, or which presented anomalies, as shown by Mr. Baily's notes to the British Association Catalogue, and other stars taken at convenient intervals to complete the number.

"The observations of the Catalogue were begun on the 1st January, 1850 (the epoch of the British Association Catalogue), and concluded on the 24th December, 1853. The observations were all made and computed by my assistant, Mr. Richard Philpott, to whom I am greatly indebted for the care and attention with which he has applied himself to the completion of the work; and the column of the greatest differences from the mean sufficiently shows that he may lay claim to the character of being a very excellent transit observer. He has been ably and diligently assisted in the computations by my second assistant, Mr. Frederic Morton, who has the charge of the equatoreal. All the computations have been performed in duplicate by these two gentlemen, and, therefore, great reliance may be placed upon all the results."

Lord Wrottesley proceeds to state that, from various causes, several of the stars selected were not obtained, in consequence of which the number on the list was finally reduced to 1009. In his enumeration of stars which were looked for without success, he appends a foot-note which deserves attention. It is expressed in the following terms:—

"I have experienced great inconvenience from the omission in the British Association Catalogue of a column giving the number in the Astronomical Society's Catalogue. There should also have been columns giving the initials of the authorities for all the places given. I hope that in a future edition these omissions may be supplied. I think that had the late Mr. Baily lived to complete the work, they would not have occurred."

The author next gives an account of the mode of ascertaining

the instrumental corrections. He gives three tables, containing the corrections for collimation, level, and deviation. He then explains the mode of reducing the observations which was pursued.

"The Catalogue is founded on the apparent places of the *Nautical Almanac* stars given in that work for the years during which the Catalogue was in course of observation.

"In observing double stars, the brightest, and in cases of equal brightness, the preceding has been observed.

"The Hardy or transit clock has behaved exceedingly well during the whole course of the observations; the rate has been remarkably steady, and the beat is, as before, loud and distinct. This is, indeed, an admirable piece of mechanism, and of all the instruments in my Observatory has given me the most unalloyed satisfaction.

"No observations have been rejected when the star has been observed on three wires, or upwards, except in one instance, alluded to in the notes, when an observation on one wire of 1378 B.A.C. was not used in the reduction; but no observations of Catalogue stars, on less than three wires, have been employed in forming the Catalogue, except in the single instance of 2091 B.A.C., which is also alluded to in the notes.

"It will be seen by the notes, that many of the stars have been often marked as faint; such stars are probably either variable, or much below the magnitudes assigned in the British Association Catalogue, and copied in column 5 of mine.

"These notes, as to faintness, have been formed on the following principle:—A note has been made directing attention to the fact, whenever, of five observations of any star, the majority; of four, the half or more; of three, the majority; and of two, the whole, have been distinguished in the observing book by the star being marked as faint, or very faint.

"Some remarks of importance as to identity and proper motion will be found in the notes. Upon the whole the proper motions now obtained agree very fairly with those previously deduced by Mr. Baily and his successors, at the time of the construction of the British Association Catalogue; but owing to the shortness of the interval elapsed, and the smallness of these quantities, as compared with the probable amount of the errors of observation, perfect reliance cannot be placed on results of this description."

The author here explains the mode of forming these notes, and concludes in the following terms:—

"I have thus brought to a conclusion my third and last star Catalogue, for I need hardly make an apology for calling that *mine*, which has been produced by a large outlay of my own capital, both material and intellectual: but I do not underrate, and trust I shall ever be willing to acknowledge, the value of the services of the able assistant, who, besides performing all the computations under my superintendence, made all the observations from whence the Catalogue has been deduced.

"I fear that I shall henceforth have still less time to spare for

astronomical labours than heretofore, and I shall devote all my resources to the completion with the equatoreal of a catalogue of double stars.

"Indeed, having passed the meridian of life, and having now many other avocations that engross a considerable portion of my time, and which are likely to augment rather than diminish, I have long arrived at the conclusion, that, however humble my powers may be, I can more effectually promote the interests of science in the way of counsel rather than action,—in the cabinet rather than the field.

"*Wrottesley, Dec.* 27, 1853."

Occultation observed at Ashurst. By Robert Snow, Esq.

Jan. 1st, 1854.

North Lat. ..... 51° 15′ 58″

West Long. ..... 1^{m} 10^{s}

Immersion of 33 *Capricorni.*

	23^{h} 41^{m} 48^{s}·5
Subtracting for Clock fast	−16·7
	23 41 31·8 True Sid. Time.

Well observed. Instantaneous at moon's dark limb. Dark limb visible.

Emersion of 33 *Capricorni.*

	0^{h} 36^{m} 30^{s}±
Subtracting for Clock fast	−16·7
	0 36 13±

At moon's bright limb uncertain. Moon and star very low.

Observations of the Nebula of Orion, made at Valletta, with the Twenty-foot Equatoreal. By William Lassell, Esq.

The following are a few extracts from this paper:—

"*Wednesday, 10th Nov.*, 1852. About 4^{h}·0 Sid. Time, turned to the nebula, power 260. A marvellous spectacle. The brilliancy of the trapezium, and of 'the 5th and 6th stars,' was most striking. The 6th seemed equally bright with the 5th, and quite as easily seen. It is removed clear away from the rays of the large star.

"It seems remarkable that in this telescope there is no difference in their visibility, while in telescopes considerably less in power the 5th star is incomparably easier to see than the 6th. I do not find so many more stars in the neighbourhood as I had expected. With 160 many sparkling points flashed on the eye by glimpses all over the nebula immediately around the trapezium; but it would be difficult to mark their places.

"With 1018 the stars of the trapezium were exhibited with fine disks; but the atmosphere was not fine enough to bear this high power upon the nebula.

"*Monday, 15th Nov.* The nebula appeared a magnificent object with 160 and 260; but I do not see many more *stars* than on some rare occasions I have seen at Starfield. The 5th and 6th stars are of equal intrinsic brightness, and seen with equal facility. The superiority of vision of these stars is the most striking characteristic of the object in comparison with the best views I ever obtained of them in England.

"*Monday, 6th Dec.* Viewed the nebula with powers 219, 260, and 1018. With the latter power a new phase was given to the nebula, which seemed like large masses of cotton wool packed one behind another; the edges pulled out so as to be very filmy.

"*Wednesday, 8th Dec.* With powers 160 and 297 the view surpassed, I think, all former ones. The brightness of the minute points about the trapezium is strikingly greater than at Starfield, yet I could not mark the places of more than three or four new stars. I afterwards applied 1018, with which there is no appearance of resolvibility. The whole aspect is that of a number of masses of fleecy cloud, thin at the edges, and packed one behind another; appearing to be a deep stratum of successive layers of nebulous substance. With this power I turned to the neighbouring star *Rigel*, which was admirably shown, the most striking feature, perhaps, being that the small star accompanying it was exhibited with a beautifully neat round disk, circumscribed by a single hair-like ring, most symmetrically formed. The large star had also a perfectly round *disk*, but the flickerings of the air prevented its being a moment still, and the rings were broken into pieces. There were also a good many sharp rays, as radii, extending rather beyond the small star, but they were of much fainter light, and did not at all interfere with the brightness or symmetry of form of the stellar disks. I was much struck with the *capacity* of the telescope for stellar purposes when the atmosphere was in its most favourable condition. On this occasion it was very slightly hazy.

"*Wednesday, 15th Dec.* The nebula with 160 was most splendidly seen. The brightness, and even *brilliancy*, of the 5th and 6th stars continues to surprise me. They are of equal magnitude and seen with equal ease, the 6th star clean removed away from the rays of *Theta*. A few more stellar points, I believe, appear than I have mapped down in my Starfield diagrams, and the stars contained in those diagrams are very much brighter. With power 1018 the wool-like masses appear as I have previously described them, and there is no disposition whatever in them to turn into stars.

"In order to perpetuate as far as possible the results of these observations, I send herewith a painting in oil of this nebula on the same scale as my original drawings, the acceptance of which I beg from the Astronomical Society. It is the work of my friend Mr. Hippisley, executed under my own superintendence, and care-

fully compared with my original sketches. I consider it a very faithful picture of what I saw when placed in a proper light and well illuminated. Without attention to these conditions the nebula will appear too faint."

Miscellaneous Observations, chiefly of Clusters and Nebulæ.

We extract from this additional paper by Mr. Lassell, the following observations:—

"*Wednesday, 15th Dec.* Surveyed *α Orionis*, power 160. A most beautiful and brilliant gem! Singularly beautiful in colour, a rich topaz; in hue and brilliancy different from any other star I have seen.

"Cluster, R.A. $5^h\ 58^m$, P.D. 65° 57′. A marvellously striking object. No one can see it for the first time without an exclamation. Power 160. The field of view 19′ in diameter, and angular subtense 53°½, is perfectly full of brilliant stars, unusually equal in magnitude and distribution over the whole area. Nothing but a sight of the object itself can convey an adequate idea of its exquisite beauty. The brilliancy and concentration of the stellar points and the blackness of the ground cannot otherwise be shown in their just contrast.

"*Thursday, 6th Jan.*, 1853. R.A. $5^h\ 33^m$, P.D. 81° 0′, H. 365. Planetary Nebula. A singular curdled-looking object, slightly and irregularly elliptical, with a sort of cordon running round parallel to, but a little outside of its margin.

"*Friday, 7th Jan. ι Orionis.* Surveyed this star for some time without any impression of a nebula about it. At length I began to conceive that the glare around it, which I had attributed to the splendour of the star, might be really nebulous; and on further looking attentively at the stars, I could fancy they were on a black ground in the midst of the nebulæ; but, without the suggestion of Lord Rosse's drawing, I think the appearance would have escaped me. When I obtained this view of it, the nebulæ seemed to be strongly tinged with a reddish hue.

"R.A. $4^h\ 7^m$, P.D. 103° 9′. Planetary Nebula. The most interesting and extraordinary object of the kind I have ever seen. A bright well-defined star, perhaps 11th magnitude, right in the centre of a circular nebula, whose edge was its brightest part; and this nebula again placed upon a larger and fainter, concentric and equally symmetrical. Power 565. A single star just within the nebula, near its preceding edge: and I had an impression of minute points of light scattered over the nebula, but none could be absolutely made out. I was in doubt whether the central star had the brightness and concentration of a fixed star proper. Certainly I should say it had not the intrinsic brightness of some of the stars about *Orion*.

"*Saturday, 8th Jan.* R.A. $7^h\ 34^m$, P.D. 104° 20′. H. 464. A glorious cluster, with a planetary nebula within it; the nebula, however, sharply defined, and apparently quite separate from the

cluster. There is some analogy in this object to the planetary nebula. R.A. $4^h\,7^m$, P.D. 103° 9′:—in this, however, there is only one stratum of nebula. Its form is circular, with a star or stellar nucleus, nearly, but not quite, in the centre. There is also another fainter star within the nebula, and rather nearer the centre than the circumference. About the third of the area around the centre is darker than the rest, *i.e.* there is less nebula there. An astonishing and interesting object.

"*Tuesday, 11th Jan.* Surveyed again the cluster and nebula. R.A. $7^h\,34^m$, P.D. 104° 20′. The star is not in the centre, and the nebula seems to retreat from the star, leaving it on a much darker ground than the external parts of the nebula. With slight attention another star is seen; and two or three points or bright spots in the nebula occasionally catch the eye. The cluster fills the field and is rich. The stars in the nebula seem dull."

Remarks on Hooke's Observations of the Comets of 1680 *and* 1682.

By the Editor.

It will be recollected that, on the occasion of the last apparition of Halley's Comet towards the close of the year 1835, a series of extraordinary physical changes were witnessed in the head of the comet as it continued to approach the perihelion. These consisted of irregular emanations of a luminous substance from the nucleus, which, after advancing a short distance towards the sun, as if attracted towards that body in obedience to the principle of gravitation, then curved backwards, as if impelled by some force of great intensity in the opposite direction. This outstreaming cone of luminous matter presented the appearance of a sector, and as such was generally described by observers. This sector was found to vary in breadth from night to night, and the axis to oscillate on each side of a mean direction, coinciding with the radius vector of the comet: even in the course of a single night the axis was observed to undergo very sensible oscillations. No parallel to these singular phenomena could be found in the observations of any comet of recent times. The great comet of 1811 presented a totally different aspect, as is manifest from the observations of Sir William Herschel and other astronomers. During its visibility the head of that comet was observed to be bounded by a bright line which extended back on each side towards the tail; but this line did not appear to have any connexion with the nucleus, nor was the latter perceived to be subjeet to those tumultuous changes which so strikingly characterised the nucleus of Halley's Comet. The only former comet mentioned by Bessel, as having exhibited analogous phenomena, was the great comet of 1744, as observed by Heinsius.

Having lately had occasion to make some researches in the writings of Hooke, I was astonished to find in the "Posthumous Works" of that philosopher, an account of his observations of the comets of 1680 and 1682, which, in so far as regards the physical

changes above referred to, tallies completely with the observations of the comet of 1744 by Heinsius, and with the numerous observations of Halley's comet by Bessel and others in 1835. The comet of 1680 was observed by Hooke with a 14-foot telescope on several evenings between the 29th of December in that year and the 10th of the following January. The description which he gives of a stream of light which was seen by him issuing from the nucleus "*in the manner of a sudden spouting of water out of an engine,*" and of the incessant fluctuations to which it was subject, exhibits a most satisfactory agreement with the records of the physical changes observed in the head of Halley's comet on the occasion of its last approach to the perihelion in 1835. The account given by Hooke respecting his observations of the comet of 1682 (which was one of the former apparitions of Halley's comet), is even still more precise on this point. He commenced his observations on the 16th of August, and continued them with more or less interruption till the 10th of September. The following is the account given by him of the appearance presented by the comet on the 26th of August. It is to be borne in mind that the passage of the perihelion took place on the 15th of the following month :—

"Aug. 26. At seven in the evening I delineated the figure and shape of the comet, exactly like that I saw through my 14-foot telescope, which will appear more plain by the fifth figure than I can otherwise well express it.* It had a pretty bright round nucleus, and about that was an atmosphere of thinner light, which was terminated towards the sun with a round figure. That part of this halo, or lighter atmosphere towards the sun, was not so bright and radiant as another kind of light *which seemed to issue from the nucleus or star both ways at right angles with the axis through the sun*; which lighter issuings bent into a kind of parabolic figure, within the former halo or atmosphere, and was terminated within it, and seemed to form, as it were, a second parabolical termination towards the sun, in the apex of which parabola was the bright nucleus, and this brighter parabolical line of light *seemed as gross or thick as the nucleus itself.* This issued on both sides, but that on the right hand, or the northernmost, was much more conspicuous; insomuch that that on the left hand, or towards the south, was to be seen but sometimes, but that on the other side was very plain and conspicuous, *and seemed like a stream of flame blown out of a candle by a blow-pipe, ascending or bending upwards, just as such a blown flame of a candle will do, if it be made by a gentle blast.* This I remarked very carefully, to see whether I could find by any succeeding observations, any alteration of the magnitude, figure, brightness, or position, in respect of the comet's axis.

"These two bright spoutings of flame or light turned or bent upwards from the sun, and after a short space seemed to unite into the axis or middle of the blaze, and form the shape of the outside of a flame of a candle, tapering to a point; the fainter part, also, without it, seemed to taper much in the same manner. I saw also several coruscations or flashings of the flame, shooting out to a great distance into the blaze."

It appears from Bessel's observations of Halley's comet in 1835, that the angular magnitude of the luminous sector, or, in other words, the angle of divergence of the luminous emanation, underwent continual variations. The following observation of Hooke's, relative to the comet of 1682, affords a confirmation of the same fact :—

* These drawings, to which he refers, are given in his "Posthumous Works."

"Aug. 29. I observed the body of it through a 14-foot telescope, and saw the spoutings out of the brighter streams from the nucleus, but especially of that on the right or northern side, which was very large and conspicuous; that on the left or south side being much fainter and less conspicuous. *They seemed not now at right angles with the axis, but to be somewhat more towards the sun, both of them; but close by the nucleus they turned about again, and extended into the blaze with a kind of tapering form, as on the 26th*, and in all other particulars it seemed to be much the same as it then appeared. For it seemed, as I said before, like a stream of light, or flame, or fusee, issuing more especially on the right or northern side, and *turning about into the blaze*, where it made a kind of tapering light; but the opposite side was not near so plain or easy to be seen."

The observations of the comet on the following evening, when compared with the above description, afford sufficient evidence of the existence of continual changes in the head and nucleus:—

"August 30th. I viewed it again with my 14-foot tube, and found its figure such as I have described in the draught. The halo or atmosphere about the head, on that side which was next the sun, *was thinner than the last time I saw it, not spreading so far that way as then*. The fusee or stream on the north side of it was very plain and bright, *but seemed not to issue now at right angles with the axis of the blaze; but, as it were, at* 45° *with it*, and then bent into the blaze with a kind of parabolic figure, and so ran parallel with the axis to a great length, and did not taper on towards it as on the 29th."

The appearance presented by the comet on the 31st of August accorded with the observations of the preceding evening, except that "the fusee or stream of light seemed now to run farther parallel to the axis into the blaze." Its aspect on the 1st of September is thus described by Hooke:—

"Sept. 1. I viewed it again with my 14-foot telescope. I could see no small stars near it; the fusee or stream of light appeared much as it had done the two preceding nights. I still plainly saw the flaring or flashing of the light, and sometimes also saw a shooting or spouting, as it were, of light from the nucleus directly into the blaze, which as quickly disappeared."

The observations of September 4 clearly indicate the existence of an *oscillation of the luminous fusee*, as was remarked by Bessel in the course of his observations of the same comet in 1835:—

"Sept. 4. The nucleus was pretty clear and round, but the blaze from it was only two emanations of a kind of parabolic figure on each side, as in the figure; but the northern side was brighter than the southern still, and they seemed to spread over one from another with an angle of 60°, so far as I could see them then; for they extended but a little way from the nucleus. The middle part of the blaze between them was hardly visible. But, as the crepusculum diminished, it appeared much brighter and fuller, and then I could see its light also in the middle part of the blaze. Scarcely any halo between the nucleus and sun appeared at first, and but little more when it appeared brightest. I observed also the flaring or flashing of the light, *and a kind of moving of the fusee on the north side*."

Hooke sums up the results of his observations of the two comets of 1680 and 1682, in a series of statements, of which the following is chiefly worthy of attention, inasmuch as it embodies the most important features of the phenomenon:—

"Secondly, that there is a gravitation *towards*, and a levitation *from* the sun. For, as I have by many observations shown, though there be a descent of the

streams from the nucleus towards the sun, yet I always plainly saw that they quickly returned, and went contrary and opposite to the sun, and that sometimes to a prodigious extent."

Compare this statement with the following by Bessel relative to the apparition of Halley's comet in 1835, and to the comet of 1744 as observed by Heinsius :—

" My observations of the comet made on the 22d of October, and the more complete observations of the comet of 1744 by Heinsius, show that the particles emitted by the comet in a direction making an acute angle with the radius vector, cease after a certain lapse of time to *approach* the sun, and then *recede* towards the tail in a direction opposite to the latter body."

Nothing can be more satisfactory than the accordance here exhibited respecting the physical changes observed in the comets of 1680, 1682, 1744, and 1835. It may be remarked that similar changes were witnessed by M. Julius Schmidt, in the head of the third comet of 1853, on the occasion of its approach to the perihelion towards the end of August last year.

Bessel remarks, that in both the comets of 1744 and 1853 the nucleus had an oval form, the longer axis of which was directed towards the sun. The observations of Hooke are not very precise on this point; but the following statement by Hevelius respecting the appearance of the same comet serves to make the account of the English philosopher more complete: — " In ipso capite, beneficio longioris telescopii, non nisi unicum nucleum figuræ ovalis et gibbosæ constanter notavimus; nisi quod die Sep. 8 ex dicto nucleo clarissimus simul radius, ex parte etiam incurvatus in caudam exiret." (*Annus Climactericus*, p. 123.) This is the only passage of the account of the comet given by Hevelius, which contains any allusion to the luminous emanation from the nucleus. As regards the ***physical changes*** which Hooke observed from night to night, the celebrated astronomer of Dantzic does not seem to have had any cognizance of their existence beyond what may be inferred from the foregoing statement.

With respect to the apparition of Halley's comet in 1835, Bessel remarks that the direction of the outstreaming luminous cone generally made an acute angle with the radius vector of the comet, whereas, in the case of the comet of 1744, it appeared to have issued from the nucleus at all angles, making at one time even a right angle with the radius vector. The comet of 1682 exhibits in this respect a close analogy to the latter comet, as is manifest from the foregoing passages which have been cited, from Hooke's account of its appearance.

A curious feature witnessed during the apparition of the comet of 1835 was the oscillation of the axis of the outstreaming luminous cone to and fro on each side of the radius vector of the comet. Bessel remarks that Heinsius makes no allusion to a similar phenomenon having been visible on the occasion of the apparition of the comet of 1744. We have seen that the observations of the comet of 1682, made by Hooke on the 4th of September, exhibited an unequivocal indication of such an oscillatory movement.

After I became acquainted with the remarkable observations of Hooke relative to the comets of 1680 and 1682, from which the foregoing passages have been extracted, I endeavoured to ascertain whether they had been alluded to by any modern writer. I expected, more especially, that phenomena of so striking a character as those recorded by the English philosopher could hardly have escaped the attention of Olbers, who was so intimately conversant with the literature of cometary astronomy. I found, in fact, that the venerable astronomer of Bremen has alluded to Hooke's observations of the comet of 1682, in a paper relative to the approaching apparition of Halley's comet, published about the beginning of the year 1835, in vol. xii. of the *Astronomische Nachrichten*, p. 57, *et seq.*, but he seems to have entirely overlooked those tumultuous changes which Hooke perceived the comet to be subject to when examined from night to night, and which serve *exclusively* to establish the analogy between its physical condition and that of the comets of 1744 and 1835. This, however, cannot excite any surprise when it is borne in mind that the paper in question was written *previously* to the last apparition of Halley's comet. The object of Olbers was to ascertain how soon the comet might probably become visible by a reference to the records of former apparitions, and he cites Hooke's observations of the comet of 1682 solely on account of the evidence of the brightness of the head which they afford. With respect to the physical aspect of the comet as recorded by Hooke he merely takes occasion to state that the head was bounded by a parabolic line of light *like the comet of* 1811, only that in the case of the comet of 1682 there was no interval visible between this bounding line and the nucleus.

Imagining that the extraordinary physical changes witnessed by Bessel and other astronomers in the head of Halley's comet on the occasion of its actual apparition in the autumn of 1835, might have had the effect of drawing the attention of the Nestor of cometary astronomy to the analogous changes observed by Hooke in the comets of 1680 and 1682, I consulted for this purpose the recently published *Correspondence of Olbers and Bessel*.* In a letter from Bessel to Olbers, dated the 25th of October, 1835, the illustrious astronomer of Königsberg gives a detailed account of the singular appearances which he had recently witnessed in the head of Halley's comet. The following passage, extracted from a long letter written in reply by Olbers to Bessel, shows that the infirmities of old age had prevented the writer from enjoying the gratification of personally observing those singular variations which otherwise might have induced him to search for a prototype of them among the records of former apparitions:—

* "Briefwechsel zwischen, W. Olbers and F.W. Bessel, in zwei Bänden," Leipzig, 1852. A copy of this interesting work has recently been added to the Library of the Society, forming one of the many valuable donations of a similar kind for which the Society is indebted to the Rev. Richard Sheepshanks.

"Since the 22d of September I have frequently regarded it from my little garden, first with a small comet-searcher by Hoffmann, and afterwards with the naked eye. On two occasions (the 18th and 22d of October) have my servants, with much difficulty, conducted me to my observing chamber, when I perceived the comet with my 5-foot Dollond instrument, but not with a magnifying power of less than forty-four. My bodily infirmities prevented me from applying a higher magnifying power or from using my beautiful 6-foot Fraunhofer. Besides, I dared not remain long exposed to the cold evening air. Of *your wonderful streams of light I have seen nothing.*"*

It is interesting to find that the theory proposed by Hooke, to account for the singular changes witnessed by him in the heads of the comets of 1680 and 1682, agrees mainly with the explanations which have been suggested by Bessel and Sir John Herschel relative to the analogous phenomena observed on the occasion of the apparition of Halley's comet in 1835. That there is an actual ejection of luminous matter from the nucleus seems to have been the firm opinion, not only of Hooke, but also of all modern observers of the same phenomena.† Hooke maintained that the velocity with which the luminous particles seemed to move from the head towards the tail of the comet could only be compared with the velocity of lightning. He supposed, therefore, that by the agency of the solar heat vapours were raised in the head of the comet; and, in accordance with the prevailing notions of his time, he then proceeds to show that the sulphureous and resinous particles, by coming into contact, produced the flame of lightning which propagated itself from the head to the tail in an amazingly short space of time.

The explanation suggested by Sir John Herschel relative to the origin of the same phenomena is contained in a foot-note appended to one of the pages of his *Results of Astronomical Observations made at the Cape of Good Hope*. It is expressed in the following terms:—

"It is prudent, to say the least, in such hypotheses, to keep clear of specialties, yet I cannot help remarking that the conception of a high degree of electrical excitement in the matter of the tail (of the same character with that of a permanent electrical charge, supposed to be resident in the sun), superadded to the ordinary conception of a gravitating nucleus, would satisfy most of the essential conditions of the problem. That the sun's heat in perihelio does actually vaporise a portion of the cometic matter, there can no longer, I think, be any reasonable doubt. That, in such vaporisation, a separation of the two electricities should be effected, the nucleus becoming (suppose) negative and the tail positive, is in accordance with many physical facts. The circumstances of this vaporisation, as they were seen to take place in Halley's comet, were highly favourable to such separation. The matter of the tail seemed to be emitted in violent jets and streams, as if from orifices or fissures in the anterior part of the nucleus, a state of things productive (as recent experiments have shown) of powerful electrical excitement." (*Results of Ast. Obs.* p. 409.)

In reference to an objection that might be urged to this hypothesis, on the ground that it supposes the sun to be in a perma-

* *Briefwechsel, Zweiter Band*, p. 406.

† "Der Comet stösst die Materie aus, darüber ist kein Zweifel." (Bessel to Olbers, *Briefwechsel, Zweiter Band*, p. 403.)

nently electrical state, and that, therefore, thunderclouds in our atmosphere ought to be attracted to or repelled from it, and move accordingly, Sir John Herschel remarks, that while the matter of a cloud is vastly more dense than that of a comet, the latter, in all probability, is more powerfully excited than the cloud; and hence, on both these grounds, the motion of the cometic particles ought to be inconceivably swifter than the motion of the cloud. The argument is still further strengthened by the consideration that the cloud moves through a medium which is infinitely denser than that traversed by the comet. It is interesting to find Hooke endeavouring to support his theory by means of an argument closely resembling that which is here employed by Sir John Herschel.*

The views entertained by Bessel respecting the origin of the luminous emanations are somewhat similar to those of Sir John Herschel, as above stated.

With respect to the remarkable appearances which Hooke observed in the comet of 1680, he states that he had never met with any mention of similar phenomena in any author or from any person who had devoted his attention to comets. He adds, however, that he had been told by "*some antient men*" who had seen the great comet of 1618, that it did perfectly sparkle and shoot fire, but he was disposed to attribute this notion to the exaggerated effect produced upon the imagination by the terrific aspect of the comet.

On the Zodiacal Light. By the Rev. T. W. Webb.

The following memoranda of former appearances of the zodiacal light may, perhaps, be thought worthy of preservation, at any rate so far as they may exhibit some correspondence with the observations of Mr. Lowe, in the *Monthly Notices* for November.

"1837, Feb. 23. I think it was about $7^h\ 30^m$, but I am not confident as to the time that I saw the zodiacal light, extending as far as the *Pleiades*, its termination, which was very faint, being, I think, a little to the south of them.

"March 7. About $7^h\ 50^m$, the zodiacal light was again visible, tolerably bright near the horizon, but faint at a greater elevation. It seemed, however, to extend as far as the *Pleiades*, and a little beneath them.

"1839, Jan. 9. The zodiacal light was very visible about $6^h\ 30^m$ p.m., but not well defined; the base was broad, and the point directed towards *Aries*. Its exact extent could not be ascertained, but by oblique vision, by which it was best seen, it appeared to extend beyond, and beneath, γ *Pegasi*.

* "And though the progress of the flash in comets be probably a thousand times more swift; yet considering the vast difference there is between the air and the ether, I know not but that the progress of the ascension of flame in the lightning may be accounted as quick through the thick medium of air, as that of the comet in the thinner and more fluid medium of ether."—(*Posthumous Works*, p. 170.)

"Jan. 17. Zodiacal light very conspicuous about $6^h\ 30^m$. It appeared by oblique vision to extend nearly as far as the stars in *Aries*.

"1842, Dec. 27. About $6^h\ 15^m$ the zodiacal light was noticed, which soon afterwards became more distinct, and was watched until after 7^h. It extended from the horizon in a direction towards, or a little beneath, the stars of *Aries*, which, however, it did not reach, becoming invisible under the quadrilateral of *Pegasus*. Its termination, however, as well as its edges, was exceedingly ill defined. It was brighter than the faintest, but fainter than the brightest portions of the galaxy, and of a different tint, not so pure and ethereal, but more inclined to yellow. The suspicion so continually recurred, as almost to give the impression of certainty, that, like the tail of some comets, it had a less luminous space in the centre. This was not noticeable in the broader base, but began to appear some degrees above the horizon, and was feebly traced throughout its length. It seemed as though the cone were composed of two converging rays, inclined to one another at an angle of about 5° or 10°, and it appeared as though the lower one, or that towards the south, were broader than the other.

"1843, Jan. 1. About 6^h p.m., the zodiacal light began to be noticed, and was watched for about 30^m. It seemed rather brighter than on Dec. 27, 1842, so as to be somewhat brighter than the more luminous parts of the galaxy. Its direction and extent, and the ill-defined character of its edges and summit, were much as upon that occasion, and the same suspicion again returned as to its being composed of two streams or rays.

"1843, Jan. 22. A beautiful zodiacal light, in a very clear evening, between 6^h and $6^h\ 45^m$, extending nearly to the stars of *Aries*; ill defined, but brighter than any visible part of the galaxy, and of a yellower tinge. The suspicion of its being double would probably not have occurred, certainly not to any one uninformed as to previous appearances.

"1843, Jan. 30. Fine but ill-terminated zodiacal light. It might easily have been fancied double by any one who wished to find such a phenomenon; but if it exists, I am sure it is less perceptible by far than on Dec. 27.

"1843, March 18. Zodiacal light noticed about 8^h p.m. as a low, broad, ill-defined cone in the west. It seemed to extend nearly to the *Pleiades*.

"1843, March 24. Zodiacal light about 8^h p.m.; a broad, conspicuous, ill-defined cone, extending to or beyond the *Pleiades*.

"1843, March 29. After evening twilight, the zodiacal light was conspicuous as a broad, ill-terminated cone, reaching further than the *Pleiades*, and probably touching the skirts of the galaxy, its configuration with which presented a beautiful and interesting spectacle. Later, there was an auroral light, somewhat further on than the place of the now fading zodiacal light, which for a short time seemed to send up a few faint short streamers. The zodiacal light was this evening considerably brighter than the Milky Way.

"1850, March 11. Broad and bright, but ill defined, and I think, patchy, zodiacal light, extending to the *Pleiades*.

"1850, Dec. 23. Between $7^h\ 30^m$ and 8^h p.m., there was much diffused light along the west horizon, extending from the setting of the galaxy 40° or 50° towards the south. It had not at all the appearance of the aurora borealis, but much more the aspect of another form of zodiacal light, a faint beam of which seemed to stream upwards through the region of *ζ Aquarii* towards *Saturn*. It struck me as being very probably a greatly foreshortened projection of this light, which, if this is the case, will shortly become visible in its lenticular form.

"1852, Jan. 17. As twilight disappeared, the zodiacal light began to be visible; fainter than the galaxy, but extending beneath and beyond the stars of *Pegasus*, and nearly as far as *Aries*; its edges and termination were, however, exceedingly ill defined. A suspicion was entertained that it consisted of several separate, though not distinct, streams. Later, perhaps about $6^h\ 30^m$, its lower and broader part was very conspicuous, twilight having now disappeared, and it there seemed to be much brighter than the Milky Way. Its further condition could not be observed.

"1853, Feb. 26. The zodiacal light was beautifully seen at and after $7^h\ 45^m$. It appeared as a pillar of diffused light, extending beyond the stars of *Aries*, but very ill defined at its sides and termination. It did not appear, at first, to be much inclined to the horizon; as it sank lower, it was fancied to be more so. Its centre appeared more luminous than its edges, and far brighter than any visible portion of the galaxy, so as to be a conspicuous object.

"March 8. The zodiacal light was again very splendid about $7^h\ 45^m$. A tapering prolongation appeared to the averted eye to extend nearly to the *Pleiades*.

"*Tretire, Dec.* 28, 1853."

On a newly-invented Portable Equatoreal. By Mr. William Rothwell.

This instrument consists of a telescope 14 inches in length, carrying the hour-circle at the eye-end, and turning on cylindrical collars in uprights securely fixed to a brass pillar attached to an horizontal plate, so that the telescope, which is the polar axis, has an inclination equal to the latitude of the place, when the plate is levelled by the three foot-screws, provided for that purpose; there is also a pair of antagonist screws which give the whole of the instrument a movement in azimuth, and serve to place the instrument upon the meridian.

Below the telescope is an horizontal axis, supported upon two uprights in the usual manner, the western pivot of which carries the declination-circle, and is furnished with a level and the usual adjustments appertaining to the finding circle of a transit instrument; this axis in rotating carries with it, directly under the telescope, a plane mirror, whose surface is adjusted parallel to the declination

axis, when its normal ought to coincide with the line of collimation of the telescope, the mode of effecting which has yet to be described. The adjustment of the line of collimation to the normal of the mirror is obtained by setting the mirror parallel to the object-glass of the telescope; then the direct rays from a lamp illuminating the wires will render their images reflected from the axis mirror visible in the field of view of the telescope, which must be made to coincide with those seen by direct vision, by means of the adjusting screws at the eye-end, in the usual manner; then reverse the axis to its Ys, and if the reflected image of the meridian wire does not coincide with that seen by direct vision, correct one-half of the difference by the collimation adjusting-screws, and the remainder by the screws at the back of the mirror (it may be observed that this adjustment, which is a permanent one, is always made by the maker of the instrument); again reverse the axis to its original position with the circle to the west, and if the coincidence of the wires be perfect, the reflecting surface will be perpendicular to the line of collimation and parallel to the horizontal axis. Therefore, if the reflecting plane be perpendicular to the line of collimation, and parallel to the horizontal axis, and also at right angles to the meridian, the axis-mirror when moved in altitude, from the horizon to the zenith, will reflect successively upon the meridian wire of the telescope all the heavenly bodies as they pass the meridian.

It is adjusted in the following manner:—

Level the base of the telescope support by means of the hanging level, which is constructed to answer both purposes.

To determine the inclination of the telescopic polar axis to the latitude, and find the horizontal point.

Elevate the telescope by raising the plate which supports it, until it is nearly horizontal; then, by placing a brass tube with adjusting steel points, provided for that purpose, into two holes, between the upper and lower brass plates, it will support the telescope in an horizontal position; apply the level to the cylindrical collars of the telescope, and if the bubble be not in the middle, screw or unscrew the steel points of the brass tube support until it be so; when the horizontal point will be obtained by direct vision; then bring the bubble of the cross level into the middle of its opening, and clamp it securely; set the declination arc to zero, and clamp it; then depress that end of the cross-level which is nearest the eye-end of the telescope, through an angle equal to the latitude of the place; remove the brass tube support and lower the plate, carrying the telescope to its original position, where it is supported at one end, on the point of an adjustable screw, firmly fixed to the lower plate; and if the cross-level of the axis be not in the middle, adjust by the screw, and the polar axis will be at an inclination equal to the latitude.

The true reflected horizontal point will be as much below that determined by direct vision as is equal to the descent of the hole in the centre of the axis, and if brought by reflection from the mirror on the horizontal wire of the telescope, the vernier set to zero, and

the cross-level clamped with its bubble in the middle, it will read altitudes: in order to read declination, elevate the mirror to the co-latitude; clamp it; set the vernier to zero, and the cross-bubble in the middle of its opening.

If the equatoreal be removed to any latitude within its limits, a revision of its adjustments will be unnecessary, as the inclination of the polar axis can be adjusted to the difference of latitude by the graduated arc at the end of the side plate, which is screwed on the moveable one.

The telescope, when in the horizontal position, can be used as a transit instrument, from the equator to within 20 degrees above the north horizon. (If the plane of the mirror be parallel to the declination axis, and perpendicular to the line of collimation, or polar axis, and the declination axis at right angles to the plane of the meridian, the polar axis will be in the last-mentioned plane), which, in equatoreals, is a desideratum.

To prove the flatness of the surface of the mirror, place it at right angles to the line of collimation, by reflection of the wires, and reverse its axis to its Ys, to verify the adjustments; then select an object in the horizon; bring it on the meridian wire; reverse the axis; and if the object be in coincidence with the wire, the mirror is a true plane.

It may be observed, in conclusion, that if the plane of the mirror be parallel to the declination axis, the axis at right angles to the line of collimation and perpendicular to the plane of the meridian, the collimation or polar axis will be in the last-mentioned plane, which, in equatoreals, is a desideratum; and that the telescope, being a fixture, the observer never changes his position in observing stars from the horizon to 20° north of the zenith.*

Stellarum Fixarum imprimis Duplicium et Multiplicium Positiones Mediæ pro Epocha 1830·0, *deductæ ex Observationibus Meridianis, annis* 1822 *ad* 1843 *in Specula Dorpatensi institutis, auctore F. G. W. Struve, fol. pp.* 380 (*Introduc.*, pp. ccliv.), *Petrop.* 1852.

This work, which contains the conclusion of the record of M. Struve's labours on Double Stars at Dorpat, is dedicated by the author "to the memory of the immortal individuals W. Herschel and F. W. Bessel," who contributed in so high a degree by their respective researches to the advancement of stellar astronomy.

M. Struve commences his elaborate Introduction with a brief history of his labours on double stars. When, in the year 1822, he commenced a series of observations of the places of the stars with the meridian circle of Reichenbach, the object which he had in view was twofold. In the first place, he was desirous of deducing the fundamental elements of astronomical science solely from his

* It is intended to send one of these instruments to the Apartments of the Society for the purpose of inspection.

own observations; secondly, he had in view to determine with precision the mean places of all the double stars hitherto known. To this end he had, on a former occasion, drawn up a catalogue containing the approximate positions of 795 double stars, chiefly discovered by Sir William Herschel, which was published in vol. iii. of the *Dorpat Observations*. Rejecting Herschel's sixth class of stars, the distances of the components of which exceeded 60″, and eliminating those which he had found to be erroneously designated as double stars, there still remained 600 stars whose positions it was required to determine.

Towards the close of the year 1826 M. Struve had succeeded in executing all the observations which were required for the two objects above specified. In the course of these labours, however, he was compelled to enlarge the scope of his undertaking; for, having in the year 1824, obtained possession of a great refractor by Fraunhofer, he found that the double stars contained in the catalogues of Sir William Herschel formed only a small proportion of the number of stars whose duplicity was exhibited in this powerful instrument. He therefore resolved to undertake a careful survey of that portion of the heavens which extends from the north pole to 15° of south declination, with the view of detecting all the double stars down to the eighth magnitude, and within 32″ of mutual distance, which were contained in that region. He achieved this task in the space of three years, and in 1827 he published the *Catalogus Novus*, which contained more than 3000 double stars. Instead of 600 stars, there were now more than 3000, whose absolute places it was required to determine, and the relative positions of whose components it was desirable to ascertain by micrometrical measurement. As it was impossible for him, individually, to accomplish both of these objects, he reserved the execution of the micrometrical measurements to himself, and assigned the observations with the meridian circle to his assistant Preuss. In 1837 he published the *Mensuræ Micrometricæ*, which contained the results of his own labours. The meridional observations were interrupted by the death of Preuss, which occurred in the summer of 1839, when M. Struve was just about leaving Dorpat in order to assume the directorship of the Imperial Observatory of Pulkowa. In 1840 they were resumed at Dorpat by M. Doellen, under the superintendence of M. Mädler, who was now Director of the Dorpat Observatory, and were finally concluded in the year 1843. The meridional observations of Struve, Preuss, and Doellon, are recorded in vols. iv., v., vi., vii., viii., ix., x., and xii. of the *Dorpat Observations*. The last of these volumes was published in the year 1850. The total number of sidereal positions determined by the three observers amounted to 27,600. The object of the present work is to assign the mean positions deducible from these observations.

After a disquisition on the special character of the observations, M. Struve next considers the constants of aberration, nutation, and refraction. With a view to determine the constant of nutation, he caused all the observations of *Polaris* made at Dorpat between the

years 1822–38, to be carefully reduced. These results formed the groundwork of the well-known investigation of M. Peters. The value of nutation thus deduced was $9''{\cdot}221 \mp 0''{\cdot}018$. The corresponding value of aberration was $20''{\cdot}442 \mp 0''{\cdot}016$.

The mean refraction was deduced from observations of 42 circumpolar stars. Of these the nearest to the pole was *Polaris* itself, and the most distant was 38 *Lyncis*, which, at its upper transit towards the south, had a zenith distance of 20° 50′, and at its lower transit towards the north had a zenith distance equal to 84° 5′. By a discussion of these observations he found the mean refraction to be $\varrho^{I} = 57''{\cdot}488 \mp 0{\cdot}030$, the barometer being supposed to stand at 332·75 French lines, and the thermometer at 7°·44 Reaumur. Bessel in the *Tab. Reg.* makes the same constant $\varrho = 57''{\cdot}665 \mp 0''{\cdot}041$. Hence the difference between Struve's determination and that of Bessel's is $\varrho^{I} - \varrho = -0''{\cdot}177 \mp 0''{\cdot}050$. M. Struve seems disposed to conclude, from this considerable discordance, that the constant of mean refraction is to a certain extent dependent on local causes, the origin of which is to be found in the situation of the thermometer, and in the form and dimensions of the observatory.

From 46 observations of the sun's declination made at the solstices, or near those points, in the years 1823–4–5, M. Struve determined the obliquity of the ecliptic for the commencement of the year 1825 to be $= 23°\ 27'\ 42''{\cdot}61 \mp 0''{\cdot}11$.

The apparent diameter of the sun, corresponding to the mean distance of the earth, was deduced from a series of observations extending over four years.

From 219 observations, the vertical semi-diameter of the sun $= 16'\ 0''{\cdot}66 \mp 0''{\cdot}037$.

From 241 transits of the limbs, the horizontal semi-diameter $= 16'\ 1''{\cdot}12 \mp 0''{\cdot}043$.

The difference between the two semi-diameters is, therefore, $0''{\cdot}46 \mp 0''{\cdot}057$. This quantity is sufficiently small to be explicable by errors of observation, but if it really exists, it would seem to indicate a compression of the sun's mass, arising from its rotatory motion.

M. Struve next gives a fundamental catalogue of 109 stars, the places of which emerged from his preceding researches, and after a critical examination of the partial catalogues of the different observers upon which the complete catalogue of the work is founded, he then proceeds to explain the construction of the latter. The whole number of stars is 2874, including the 109 fundamental stars whose positions he had already given. The number of double stars in the Catalogue is 2682. Of these 2511 are contained in the *Catalogus Novus*. The epoch of the Catalogue, as mentioned in the title-page, is 1830·0.

M. Struve next institutes a comparison between the results contained in his Catalogue and those of the earlier Catalogues of Bradley, Piazzi, Lalande, and Groombridge. His [illegible] right ascensions may be completely reconciled [illegible] and Groombridge by applying a correction of [illegible]

tion of the meridian mark at Palermo, and a correction of $+2''.44$ to the meridian mark of Blackheath.

The following probable errors of a single star for the Catalogues of Groombridge, Piazzi, and Lalande, are given by the author:—

	In R.A.	In Decl.
Groombridge	$0''.95 \sec \delta$	$0''.72$
Piazzi	$1''.50 \sec \delta$	$0''.97$
Lalande	$3''.23 \sec \delta : \sqrt{\sigma}$	$2''.03 : \sqrt{\sigma}$

where σ denotes the number of observations of the same star to be met with in Lalande's Catalogue. Hence, if we denote the error of a single observation of a star in Lalande's Catalogue by unity, we have the following numbers expressive of the relative precision of the position of a star in each of the three Catalogues:—

	Rel. Precis. in R.A.	Rel. Precis. in Decl.
Groombridge	3·1	2·8
Piazzi	2·2	2·1
Lalande	1·0	1·0

After a discussion of the subjects of precession and of the motion of the solar system in space, M. Struve next considers the proper motions of the stars. In reference to this question, he states that his General Catalogue contains 386 stars, which are to be found also in Bradley's Catalogue, as exhibited by Bessel in the *Fundamenta*. Of these 123 are common to Argelander's Catalogue of 560 stars. Sir John Herschel has regretted that no materials exist in the present day for ascertaining the proper motions of telescopic stars. With respect to this question, M. Struve remarks that his Catalogue contains 401 stars, which are visible to the naked eye, and 2473 smaller stars. Of the latter number there are more than 1400, which are of the eighth magnitude, and downwards to the tenth magnitude. M. Struve justly states that his Catalogue, in consequence, offers precious materials for investigating the proper motions of the smaller stars, more especially double stars, by a comparison of its results with observations made at some future epoch. He adds that the preparation of such materials for the use of posterity was the main object which he had in view in determining the absolute positions of telescopic stars.

But, in order to avail himself as largely as possible of all existing materials, M. Struve compared his own Catalogue, not only with Bradley's, but also with the later Catalogues of Piazzi, Lalande, and Groombridge. This operation furnished 1276 additional stars, whose proper motions are indicated with a greater or less degree of certainty.

M. Struve divides the stars of Bradley into two classes. B^a contains 180 stars, which are estimated to have a mean magnitude equal to 3·15, and a mean distance equal to 2·88 (the mean distance of a star of the first magnitude being supposed equal to unity). B^b contains 206 smaller stars, whose mean magnitude and distance are estimated at 5·66 and 7·74 respectively. The remaining

list of stars R, to which proper motions are assigned, and which have been already stated to amount to 1276 in number, are estimated to have a mean magnitude equal to 7·34, and a mean distance equal to 18·15. The whole number of stars which formed the subject of research amounted, therefore, to 1662, of which there were 734 single stars and 928 double stars.

The following results relative to the proper motions of the three classes of stars above mentioned are given by M. Struve, in which λ signifies the reciprocal of the mean distance of each class:—

		Mean Motion in 30 Years.		
	λ	R.A.	Decl.
		″	″
From 180 stars, B^a	0·4077	4·64 ∓ 0·52	4·58 ∓ 0·52
— 206 — B^b	0·1326	1·87 ∓ 0·16	1·41 ∓ 0·15
— 1276 — R	0·0612	1·12 ∓ 0·29	0·82 ∓ 0·16

M. Struve closes his researches on the proper motions of the stars by summing up the most important conclusions deducible from them.

The first of these conclusions is, that the proper motions of the different classes of stars are nearly proportional to the distances which have been assigned to them.

Since the angular motions of the different classes of stars thus appeared to be proportional to their distances, M. Struve hence infers that their linear motions are equal. This he found to be true for stars situate in every direction of the heavens, by a comparison of the proper motions of the stars in the different hours of right ascension.

The following table, given by M. Struve, is founded on an examination of the proper motions of the stars of different magnitudes, as deduced by him:—

	Mean Motion in 100 Years.			
	Isolated Stars.		Binary Stars.	
Magnitude.	R.A.	Decl.	R.A.	Decl.
	″	″	″	″
1·0 M*	34·2	29·0	55·5	47·0
2·0	18·9	16·1	30·8	26·1
3·0	12·4	10·5	20·1	17·0
4·0	8·7	7·4	14·2	12·0
5·0	6·3	5·3	10·2	8·6
6·0	3·7	3·1	6·0	5·1
7·0	2·2	1·8	3·5	3·0
8·0	1·4	1·2	2·3	2·0
9·0	1·0	0·9	1·7	1·5

From the circumstance of the proper motion, exhibiting a gradual diminution, according to the brightness of the star, M. Struve concludes that the stars of the seventh, eighth, and ninth magnitudes, are more especially adapted for investigating the motion of

* The letter M refers to the magnitudes as estimated in the *Mensuræ Micrometricæ*.

precession than the brighter classes of stars which are usually employed for that purpose.

With respect to the proper motions of double stars, M. Struve finds that they exceed those of isolated stars in the proportion of 1·624 to 1. He also finds that *those binary stars which revolve more quickly in their orbits are remarkable for their large proper motion.*

M. Struve finally concludes from his researches on this subject, that the proper motion of the sun is greater than $\frac{5}{10}$ths, and less than $\frac{8}{10}$ths of the mean proper motion of 734 single stars, whose positions at different epochs he had compared together.

From the proper motions of the stars, M. Struve passes to the consideration of the relative motions of double stars. These may be subdivided into two classes, viz., those whose constituent bodies have a physical connexion, and those which are merely optically double. M. Struve points out three criteria for deciding the question of the existence of a physical connexion in the case of a double star. First, the revolution of the two bodies around their common centre of gravity furnishes an incontestable demonstration of the existence of a physical connexion. The number of double stars, however, which admit of being subjected to this criterion is very small, hardly amounting to so many as twenty. Secondly, the existence of a physical connexion may be established by the aid of the doctrine of probabilities. In this case, again, the data are so uncertain that little confidence can be placed in the results, even if the principles of the method were absolutely unobjectionable. Lastly, the existence of a common proper motion of the two constituent bodies affords conclusive evidence in favour of their physical connexion. This is a more fertile criterion than the first, and a more trustworthy one than the second; and it has accordingly been very extensively employed by M. Struve.

From a comparison of the absolute and relative proper motions of 208 pairs of stars, M. Struve finds that there are 168, the constituent members of each of which have a common proper motion, and that with respect to the remaining 40 pairs, the relative motions of the components may be explained in each case by the proper motion of one of the two stars. The following table exhibits the results of M. Struve's researches on this subject, the stars being divided into classes depending on the mutual distances of the components.

Class.	Mut. Distance.	No. of Stars. Total No. n	Phys. Doub. a	Opt. Doub. b	$\frac{a}{n}$	$\frac{b}{n}$
	″ ″					
I.	0 and 1	9	9	0	1·000	0·000
II.	1 — 2	16	16	0	1·000	0·000
III.	2 — 4	43	43	0	1·000	0·000
IV.	4 — 8	34	32	2	0·941	0·059
V. & VI.	8 — 16	32	28	4	0·875	0·125
VII. & VIII.	16 — 32	34	23	11	0·676	0·324
IX.	32 — 64	21	10	11	0·476	0·524
X.	>64	19	7	12	0·368	0·632

M. Struve has endeavoured to ascertain by an inductive process founded upon these results, the relative number of physically double stars in the northern hemisphere. The conclusion at which he arrives, by a train of very ingenious reasoning, is expressed by him in the following terms:—"In the northern hemisphere there are about 72500 stars of the magnitudes 1 to 8·5 M. Of these there are 1973 double stars, the extreme distance of the components being supposed equal to 16″, and the limiting magnitude of the companion being estimated at 11·0 M. One in every 36·7 stars is consequently a double star. Of the 1973 recognised double stars, 1702 have a physical connexion, and 271 are merely optically double, or among every seven double stars there are six which have a physical connexion, and only one whose duplicity is merely apparent:—*a physical connexion of the constituent bodies is, therefore, not the exception but the rule.*"

M. Struve remarks that with respect to the brighter classes of stars, the comparatively large number of physically double stars to be met with among them arises from their being so easily resolved, and from the circumstance of their proper motions being so well known. In accordance with this view of the subject, he proceeds to examine more especially the number of physically double stars which are to be found among the class of stars comprised between the first and fourth magnitudes, and he arrives at the conclusion that every fourth of such stars in the heavens is physically double. He even ventures to assert, that, when we have acquired a more complete knowledge of double stars, it will be found that every third bright star is physically double. Applying these considerations to the stars of inferior orders of magnitude, he finally arrives at the following conclusion, which he admits to be of an unexpected character:—"*The fixed stars which are observable in the heavens are either isolated stars like our sun, or systems composed of two, or in some instances of several suns connected together by some physical relation. The number of isolated stars is, indeed, greater, but only three times, perhaps not more than twice the number of compound systems.*"

From the elaborate Introduction to his work, M. Struve proceeds to the various catalogues to which it refers. First, he gives the partial Catalogues, and then the General Catalogue. The latter occupies 58 pages. It exhibits the mean positions in right ascension and declination of 2874 stars, as already stated, reduced to the epoch 1830·0. The quantities of precession in right ascension and declination, corresponding to the epoch of the Catalogue, are also given for each star.

The General Catalogue is followed by a comparison of its results with the earlier Catalogues of Bradley, Lalande, Piazzi, and Groombridge. Next comes a Catalogue containing the mean positions of 175 stars, deduced from observations made by Sablaer at Pulkowa, and reduced to the epoch 1851·0.

As it appeared from subsequent researches [illegible] which had been employed in reducing both the [illegible] Catalogues

and the General Catalogue, required a slight alteration, a table is inserted after the Catalogue above mentioned, containing the small corrections which should be applied to the mean places of the stars in the General Catalogue in consequence of this modification of the elements of reduction.

M. Struve concludes his great work with a comparison of the places of 140 stars which are common to the General Catalogue, and to Argelander's Catalogue of 560 stars, which, like the former, is reduced to the epoch 1830·0.

R. G.

The *Berliner Jahrbuch* for 1856 has recently been published. The Appendix to it contains a translation of Adams' Paper on the Corrections of the Moon's Parallax, as given in the Appendix to the *Nautical Almanac* for the same year. There are also two papers by M. Encke, the Superintendent of the Ephemeris. One of these includes a set of tables for computing the right ascension and declination of a celestial body from its longitude and latitude, and *vice versâ*; the other contains an investigation of the degree of approximation which is attained in calculating the orbit of a comet by the method of Olbers. The Appendix also contains an ephemeris of each of the newly discovered planets either for 1854 or 1855, and a synopsis of the elements of their orbits.

With respect to the drawing of *Saturn* by Mr. Dawes, which appears in the *Monthly Notice* for November 1853, it may be as well to mention that the engraver has made the shading of the belts too deep, and that the dark line upon the inner bright ring should have been omitted (the engraver having been misled by what appeared to be a faint line on the original drawing), as no such indication of a subdivision of the ring is referred to by Mr. Dawes in his account of the appearance of the planet.

On the Aerolites of August 1853.

(*Extract of a Letter from M. Duprez to M. Quetelet, dated Ghent, Aug.* 12, 1853.*)

"During the nights of the 9th and 10th of August, and in a part of the sky which was entirely free of clouds, comprised between NN.E. and S.E., which might be about a sixth, I observed 29 aerolites during the first evening, and 37 during the second. They were distributed as follows:—

	h m		h	9th.	10th.
From	9 50	to	10	3	2
	10 0		11	12	15
	12 0		12	14	20

"Among the aerolites observed on the 9th there is one, the apparition of which has been accompanied by circumstances which

* Bulletin de l'Académie des Sciences de Belgique.

I consider to be worthy of mention. This aerolite appeared about $10^h\,35^m$ in the vicinity of the head of the Great Bear. It presented the aspect of a globe of fire, and was unaccompanied by a train of light. In brightness and dimensions it resembled the planet *Jupiter*. Its motion was sufficiently slow. During its apparition it diminished in brightness to such a degree that for an instant it disappeared completely, as if concealed by a kind of whitish smoke, which detached itself from it. It might have been said that the meteor in question experienced a veritable combustion.

"It appears from the directions of the various aerolites on both nights that the predominant direction was from N.E. to S.W. I directed my attention to the existence of a centre of radiation, and found, as in the case of former apparitions, that this centre was situate between the constellations of *Perseus* and *Cassiopeia*; for, of the total number of 66 aerolites, which were observed, 50 appeared to me to diverge sensibly from a point included between these two constellations."

Extract of a Letter from M. Otto Struve to the Astronomer Royal, dated 1854, *Jan.* 17.*

"Your letters have been duly received, and gave us particular satisfaction, especially on account of the prospect you hold out therein, that the astronomical part of the suggested Great Arc of Longitude might be executed in England and France without serious difficulty. On our part the operations go on very regularly. It has been decided, only a few days since, that the geodetic part, under the direction of the Imperial General Staff, shall be finished in the course of the present year, so far that our triangles shall have reached a point on the Caspian Sea, on the parallel of Marennes. It then remains only for the next year, 1855, to execute a chronometrical expedition between Karatz, Saratov, and the point in question on the Caspian Sea, to finish altogether our part of the Arc of Longitude. One year later (1856), most probably a proposed triangulation along the river Volga will also be finished, and then we shall have, for control, a second arc of longitude between Saratov and your Valentia, which, perhaps, might prove the more important, as probably the triangulations along the northern arc will be found more trustworthy and accurate than those which pass through the Southern countries of Europe.

"My father is at present actively engaged with the arrangements for the publication of the materials furnished by the measures of the great Russo-Scandinavian Meridian Arc. I think the printing will begin within two weeks; but, on account of the great quantity of materials, it will not be finished before the next year. On this

* The letter, of which the above is an extract, was received from the Astronomer Royal just as the last sheet of the *Monthly Notice* was [redacted] printed off. Additional extracts from it will be inserted in the [redacted] March.— R. G.

occasion allow me to ask you if there is any prospect that, during the pending interval, the great English Arc and Maclear's Arc shall issue from the press, so that at once all existing measures might be employed for a definitive deduction of the figure of the earth?

"I do not know if I have written to you before, that a previous comparison of the Russo-Scandinavian Arc with the East Indian, indicates a considerable augmentation of both the principal elements, the mean value of the degree and the ellipticity, as deduced by Bessel.

"Of course the definitive results can only be derived from the combination of all existing measures."

ERRATUM.

Vol. XIV., p. 18, line 3 from top, *for* and the western, *read* on the western.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIV. *February* 10, 1854. No. 4.

THE Annual General Meeting of the Society, G. B. AIRY, Esq., President, in the Chair.

Capt. H. Toynbee;
Arthur B. Martin, Esq., 43 Albion Street, Hyde Park;
Rev. Josephus Glover, M.A., Grosvenor College, Bath;
W. H. Besant, Esq., St. John's College, Cambridge;
Charles Roberson, Esq., 51 Long Acre; and
S. H. Wright, Esq., Royal Mail Steamship Lady Jocelyn,

were balloted for and duly elected Fellows of the Society.

Report of the Council to the Thirty-fourth Annual General Meeting.

The Council have once more the satisfaction of presenting to the Fellows a report of their proceedings, and of some other matters in which the Society must take interest, during the past year.

The Report of the Auditors, subjoined, will show the state of the finances:—

RECEIPTS.

	£	s.	d.
Balance of last year's account	476	6	7
By dividend on £2575 18*s.* 9*d.* 3¼ per Cents	40	12	9
By ditto on £1650 Consols	24	0	7
By ditto on £2575 18*s.* 9*d.* 3¼ per Cents	40	12	9
By ditto on £1650 Consols	24	0	7
On account of arrears of contributions	58	16	0
101 contributions (1853–54)	212	2	0
3 ditto (1854–55)	6	6	0
5 compositions	105	0	0
16 admission fees	33	12	0
12 first year's contributions	19	19	0
Sale of Publications	66	9	0
	£1107	17	3

EXPENDITURE.

	£	s.	d.
Cash paid Mr. Basire, engraver	36	6	3
George Barclay, printer	87	6	7
Cheque returned	2	2	0
George Barclay, printer	49	3	6
Executors of the Rev. Charles Turnor	50	0	0
George Barclay, printer	147	1	10
Investing compositions of Dr. Roxburgh and P. Kernan, Esq.	42	0	0
Taxes { 1 year's property tax ..... 1 9 2 / 1 year's land and house tax 5 4 2	6	13	4
J. Williams' salary	100	0	0
Ditto commission on collecting £395 2s.	19	15	0
Charges on books, and carriage of parcels	2	16	0
Postage of letters and Monthly Notices	38	6	2
Porter's and charwoman's work	12	0	2
Tea, sugar, biscuits, &c. for evening meetings	13	13	0
Coals, candles, &c.	12	18	6
Wages of boy to attend the door	9	10	0
Sundry disbursements by the Treasurer	20	1	0
Balance in the hands of the Treasurer	458	3	11
	£1107	17	3

Assets and present property of the Society :—

	£	s.	d.
Balance in the Treasurer's hands	458	2	11
1 contribution of 6 years' standing 12 12 0			
4 ——— of 5 ditto 42 0 0			
3 ——— of 4 ditto 25 4 0			
4 ——— of 3 ditto 25 4 0			
9 ——— of 2 ditto 37 16 0			
26 ——— of 1 ditto 54 12 0			
	197	8	0
Due for publications of the Society	2	1	6

£1650 3 per Cent Consols.
£2575 18*s.* 9*d.* 3¼ per Cent Annuities.
Unsold publications of the Society.
Various astronomical instruments, books, prints, &c.

Stock of volumes of the *Memoirs* :—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	40	VII.	223	XVII.	251
I. Part 2	83	VIII.	210	XVIII.	264
II. Part 1	101	IX.	217	XIX.	281
II. Part 2	63	X.	228	XX.	285
III. Part 1	130	XI.	238	XXI. Part 1 (separate).	340
III. Part 2	150	XII.	245	XXI. Part 2 (separate).	68
IV. Part 1	152	XIII.	264	XXI. (together).	222
IV. Part 2	165	XIV.	449		
V.	180	XV.	277		
VI.	198	XVI.	263		

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patronesses, and Honorary.	Total Fellows.	Associates.	Grand Total.
February 1853	133	184	64	6	387	58	445
Since elected	5	13	...	...	18	3	21
Deceased	...	−6	...	...	−6	−3	−9
Removals	2	−2	...	...	...	...	...
Resigned	...	−3	...	...	−3	...	−3
Expelled	...	−4	...	...	−4	...	−4
February 1854	140	182	64	6	392	58	450

The instruments belonging to the Society are now distributed as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Owen* portable quadruple sextant,
The *Beaufoy* circle,
The *Herschelian* 7-foot reflector,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The *Lee* circle,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,

are in the apartments of the Society.

The Brass quadrant, said to have been *Lacaille's*,

is in the apartments of the Royal Society.

The Standard scale

is in the charge of the Astronomer Royal, with the consent of the Council, to be employed in the construction of a new Standard Measure, under the direction of the Standard Committee.

The remaining instruments are lent, during the pleasure of the Council, to the several parties undermentioned, viz.:

The *Beaufoy* clock,
The two invariable pendulums, } to the Royal Society.
The *Wollaston* telescope, to the representatives of the late Professor Schumacher.
The Variation transit (late Mr. Shearman's), to Mr. Gravatt.

The Council have received the amount of the legacy left by the late Mr. Turnor for the augmentation of the library, as stated in the last Report. The proceeds of the Turnor Fund will be applied according to the testator's desire, as occasion shall arise. It is hoped that Fellows who become aware of the exposure to sale of copies of valuable astronomical works, ancient or modern, which are not in the library, will communicate the name of the work and the place of deposit to the Assistant Secretary.

The medal has been awarded to M. Rümker, for his large and laborious catalogue of stars, now happily completed. The President will state the grounds of this award at the close of the meeting, in the usual way. M. Rümker's long and useful activity as an astronomer will probably cease with this work, but the Council trust that he will be spared to see the effects of his labours, and to enjoy the progress of the science to which his life has been devoted.

The printing of the twenty-second volume of the *Memoirs* of the Society has been completed within the last few days. The communications, although not numerous, are of a more than usually diversified character. The Council would especially call the attention of the Society to a paper on the observations of the late Rev. Thomas Catton, of St. John's College, Cambridge. These observations relate chiefly to eclipses of *Jupiter's* satellites, occultations of stars by the moon, and other similar phenomena. As they were found to extend over a period of forty years, and were apparently executed with great care, it was considered desirable to put them into a shape which would render them generally accessible to astronomers. The expense attending the reduction and printing of these interesting memorials has been defrayed out of a fund placed by the Government at the disposal of the Royal Society. The operations connected with both these objects were entirely superintended by the Astronomer Royal, who also drew up the communication embodying the results, and introduced it with a luminous preface. This is only one of many similar occasions on which the Council have experienced the gratification of acknowledging the personal exertions of Mr. Airy in rendering available for scientific inquiry materials which, notwithstanding their intrinsic value, might otherwise have either perished entirely or have slumbered for ages in the archives of some public institution.

The volume of the *Memoirs*, which has just been completed, contains also a paper on the theory of clock escapements by Mr. Bloxam, of Madeira, respecting which a short statement is desirable. Upon an examination of the paper, as originally communicated by the Astronomer Royal on the part of the author, it was found to possess much merit, but still it was not considered by the Council to be of so purely an astronomical a character as to justify them in publishing it in its actual form. In this emergency Mr. Airy very obligingly undertook to adapt the paper for publication and to superintend its progress through the press. Practical astronomers

will thus have an opportunity of consulting a valuable investigation of a very important subject.

With respect to the *Monthly Notices*, the present Editor has uniformly endeavoured to adhere to the plan of publication which Mr. Sheepshanks introduced about a year since with the approval of the Council. The expediency of modifying to a certain extent the plan hitherto pursued was suggested to Mr. Sheepshanks by a consideration of the large and growing space which was being devoted to observations and ephemerides of the minor planets, arising from the continual increase of the number of those bodies by new discoveries. If, indeed, no other channel existed for the publication of these materials than that afforded by the *Monthly Notices*, the benefits which might be expected to accrue to astronomy from giving them forth to the world would have served in some degree to compensate for their effect in excluding other subjects of more varied interest. It was, however, universally admitted that all communications of this nature were published with greater regularity, as well as earlier and more fully, in the *Astronomische Nachrichten*, and that the pages of the latter periodical were familiar to all those who were in the habit of consulting such materials. The discovery of four additional minor planets since the last Annual Meeting has only served to strengthen the views of the Council in favour of the modification recently introduced. By imparting a more varied complexion to the subject-matter of the *Monthly Notices*, it is confidently expected that the interests of astronomical science will be more effectually promoted. Even as regards the minor planets, the theory of the movements of those bodies has given rise to a train of interesting researches which it will manifestly be desirable to record. On this and every other subject of astronomical science the *Monthly Notices*, under the present arrangement, offer a channel of communication, the advantages of which cannot fail to be generally appreciated.

The present Editor has gladly acknowledged to the Council the deep sense which he entertains of his obligation to Mr. Sheepshanks, who has most effectually aided him in the performance of his duties by kindly and liberally favouring him with his valuable counsel on many occasions when, from his own inexperience, he might naturally have felt some hesitation in deciding upon the course which was best calculated to advance the interests of the Society.

It is with sincere regret we have to announce the loss of Dr. Petersen, who died at Altona on the 3d of February. An account of this excellent and indefatigable astronomer must necessarily be postponed till next year; but we may be allowed to hope that the Altona Observatory will still be maintained in a state of activity and usefulness. We have not heard who is to undertake his department in the editorship of the *Astronomische Nachrichten*, but we conceive that, among the many highly cultivated and intellectual astronomers which Germany can boast, some one will be found possessed of the qualities requisite for the discharge of these

duties. In the present state of astronomy, scarcely any injury could be more serious than any delay or irregularity in the publication of this universal journal, which connects the observers of every country, and collects into one accessible whole the various improvements and discoveries which are continually making.*

The Council have also to regret the loss, by death, of three Associates:—M. Arago; Don Cerquero; Sears Cook Walker, Esq.; and of the following Fellows:—Edmund J. Dent, Esq.; George Frost, Esq.; Henry Harvey, Esq.; Lieut. Stratford, R.N.; Thomas Weddle, Esq.

Jean François Arago was born on the 26th of February, 1786, at Estagel, near Perpignan, a town in the south of France. His father, who inherited a small patrimony, was Treasurer to the Mint at Perpignan. François was the eldest of a family of five children, consisting of three sons and two daughters. In early life he contracted a strong predilection for a military life, an avowal of which on one occasion had the effect of determining his future career as a philosopher. Happening to meet an officer of Engineers on the ramparts of Perpignan, he inquired of him by what means he might become qualified to wear a similar uniform. The officer told him that he would require to study the military art at the Polytechnic School. Accordingly, soon afterwards, when he was still very young, he repaired to Paris, and entered the Polytechnic School, where he soon distinguished himself by his extraordinary vivacity and intelligence. At this time the French Revolution had occurred, and a new system of education was generally introduced throughout France, from which the study of classical literature was almost wholly excluded. It happened, in consequence, that Arago did not enjoy the advantages of a good classical education, a circumstance which he regretted very much during the subsequent period of his life.

In 1803, he entered the University of Thoulouse, where he made such rapid progress in the study of mathematics and physical science, that, at the close of a year, he was induced, at the suggestion of the elder Monge, to attach himself to the Observatory of Paris, which was then directed by Bouvard.

In 1804, being the year of his entrance into the Observatory, Arago was appointed by Laplace to co-operate with Biot in a series of experimental researches for determining the refractive powers of different gases. Biot was also at this time an assistant at the Observatory, but he was older than Arago by twelve years. The results of their joint labours were embodied in a paper which was communicated in both their names to the Institute of France, and which was published in the Memoirs of that body for the year 1806. The paper was entirely drawn up by Biot, to whom these experimental researches must be considered as mainly due. Arago,

* Until further arrangements are made, Professor Hansen undertakes the sole direction of the *Nachrichten*.

however, by his activity and intelligence, made such a favourable impression upon his elder colleague, that he was received by him into his house and treated as a member of his family.

In 1806, MM. Biot and Arago were appointed by the French Government to execute the geodetical operations required for prolonging the French Arc of the Meridian from Barcelona, in Spain, to Formentera, one of the Balearic Isles. During the latter period of that year and the beginning of the following year, they succeeded in connecting the kingdom of Valentia with the Island of Yviça, by means of an immense triangle, the spherical excess of which amounted to 39″. This had hitherto been regarded as a hazardous operation, of the success of which Méchain had altogether despaired.

The triangulation was now rapidly continued on to Formentera, the southern extremity of the Arc. Upon their arrival here, however, they found themselves unable to determine the latitude, in consequence of the repeating circle having been accidentally broken in the course of their expeditionary journeys. Biot was induced, in consequence, to return to France for the purpose of obtaining another instrument, leaving Arago and the Spanish Commissioners, Rodriquez and Chaix, to execute the triangulation of Valentia. During the summer of 1807, Arago was employed in this operation, in the execution of which, says M. Biot, he displayed uncommon ability as an observer and great powers of endurance, having been exposed to devouring heats, the effects of which his southern origin alone enabled him to withstand.

In the autumn of 1807, M. Biot returned to Spain, and in concert with Arago determined the latitude of Formentera. The facility of connecting their operations with a small arc of parallel having been recognised, they resolved to measure the latter, but for this object two observers were no longer necessary. Biot accordingly proposed to Arago either to return to France, or to remain in Spain and complete the additional operations which they had decided upon executing. Arago chose the latter alternative, and Biot returned to France.

While Arago was engaged in the operations connected with the determination of the arc of parallel, the war between France and Spain broke out, and he became henceforth exposed to great danger. Rumours were spread abroad that the fires which blazed at the signal stations on Mount Galatzo were telegraphic messages to the invading army, and it was even alleged that the astronomical instruments were pregnant with danger to the country. Arago was accordingly denounced as a spy, and he was saved from the fury of the multitude only by the authorities imprisoning him. On the 2d of June, 1808, he escaped to Algiers, where he remained ten days, and then embarked for France in an Algerine frigate. The vessel had reached the Gulf of Lyons, and was already within sight of the coast of Provençe, when it was captured by a Spanish privateer and conducted to Rosas. Arago, dreading that he might be recognised by persons who had formerly seen him in Spain, was

induced to assume the character of a pedlar. He was first confined in a windmill, but he was subsequently imprisoned along with the crew of the Algerine vessel in the Fort of Trinity, from which, after a short confinement, he was transferred to the hulks of Palamos, where he was subjected to much cruel treatment, having almost perished for want of food.

The Dey of Algiers, upon becoming acquainted with the capture of one of his vessels by a privateer of Spain, demanded satisfaction of the Spanish Government, insisting at the same time upon the immediate liberation of the crew of the vessel, including Arago. It has been said that he was prompted to this spirited act more especially by the circumstance that there had been two lions on board the vessel which were intended as a present to Napoleon, and one of which had been killed by the Spanish sailors.

Arago, having been thus liberated by the intercession of the Dey, again embarked for Marseilles on the 28th of November, 1808. Once more, however, his hopes of returning to his native country were frustrated. Just as the ship was about to enter the harbour of Marseilles, a sudden storm drove it out to sea and carried it to the coast of Sardinia, which was then at war with Algiers. The vessel, although much shattered, dared not make for the land, but directed its course towards the coast of Africa, which it reached on the 5th of December, entering a place called Bougia, about three days' journey from Algiers. Arago arrived at Algiers on the 25th of December. The Dey, who had rescued him from the hulks of Palamos, had been beheaded in his absence. The new Dey opposed his departure, believing him to possess great wealth; and he was saved from a cruel death only by the interference of the Danish Consul. Fortunately the Dey was soon afterwards executed, and Arago, after a residence of six months, quitted Algiers on the 21st of June, 1809. On the 1st of July, when the vessel was within sight of Marseilles, it was chased by an English cruiser, but fortunately it escaped, and Arago safely arrived in his native country, bringing with him all his instruments, manuscripts, and plans.

The friends of Arago, who had received no intelligence respecting him since the return of Biot, believed him to be dead. His mother, during his absence, had succeeded in obtaining possession of the watch which he had been obliged to sell at Rosas; but she had despaired of ever more seeing her beloved son. The first letter which Arago received, while he was yet in the Lazaretto of Marseilles, was one from the illustrious Humboldt. Then commenced a close friendship between these two distinguished men, which continued without interruption till death put an end to it.

Upon the arrival of Arago in Paris, he was received with great attention by Laplace, Monge, and many of the other *savans* who then adorned the metropolis of France. In 1809, when he was only twenty-three years of age, he was elected a Member of the Institute of France in the room of Lalande, whose death had occasioned a vacant place in the section of astronomy. Soon after-

wards he was appointed one of the Professors of the Polytechnic School, and also one of the astronomers attached to the Imperial Observatory.

The discovery of the polarisation of light by Malus, in the year 1811, had the effect of directing the attention of Arago to that interesting branch of optical science. His brilliant labours on this subject, and his subsequent researches on magnetism, will always secure for him a high place among the successful cultivators of experimental physics. It would be unsuitable, however, to give any detailed account of them on the present occasion.*

In 1816, Arago, in conjunction with Gay-Lussac, established the *Annales de Chimie et de Physique.* In the same year he visited England, when he became personally acquainted with Dr. Young, and several other distinguished philosophers of this country. In 1818 he was appointed along with Biot to execute the geodetic operations required for connecting the French Arc of the Meridian with the English Arc. In 1821, the results of their joint labours in Spain, and on the present occasion, including Biot's pendulum experiments in Scotland, were published in a work entitled, "*Recueil d'Observations Géodésiques, Astronomiques et Physiques, exécutées par ordre du Bureau des Longitudes.*" It is right to mention that the text and the calculations of this work are exclusively due to Biot. It may be stated, also, that the work does not contain any account of Arago's operations relative to the measurement of the arc of parallel in Spain, to which allusion has been made in a former part of this notice.

In 1822 Arago was appointed a Member of the *Bureau des Longitudes.* Henceforward he continued to publish in the *Annuaire* of that body a series of scientific notices relative to different subjects, which, besides being remarkable for the lucid and charming style in which they are expressed, are highly valuable on account of the light which they throw upon many interesting points of physics.

In 1830, when the revolution of July broke out, M. Arago, who had always been an ardent friend of liberty, gave his cordial adhesion to the movement party. He was consequently elected member of the Chamber of Deputies for the Lower Seine, and thus became involved in the strife of political warfare. In the same year he was appointed Director of the Observatory of Paris, and also Perpetual Secretary of the Institute for the Mathematical and Physical Sciences in the room of M. Fourier.

Although gifted with great powers of eloquence, M. Arago seldom spoke in the Chamber of Deputies except when advocating some question relating to the interests of science. On every such occasion his opinion was listened to with profound deference, and was generally effectual in persuading the Chamber to sanction the

* The reader is referred for an account of M. Arago's experimental researches, and also for further details of his life, to an article in the *North British Review* for the present month (February), drawn up, it is understood, by a personal friend of the deceased.

project recommended by him. It was mainly by his influence that the Observatory of Paris was repaired and furnished with new instruments, and thus placed in a condition of rivalling the other great Observatories of Europe. He was also instrumental in procuring the publication of new editions of the works of Laplace and Fermat at the expense of the country. In short, every project of a scientific nature—whether relating to engineering, surveying, telegraphs, or any similar object, which recommended itself for adoption on the ground of its practical utility, found in M. Arago an indefatigable and eloquent advocate.

As Perpetual Secretary of the Academy it became the duty of M. Arago to write the *éloges* of eminent deceased Members and Associates of that body. His compositions of this nature are all remarkable for the chaste and eloquent language in which they are clothed.

In 1834 M. Arago visited England a second time, and attended the meeting of the British Association for the Advancement of Science, which was held that year in Edinburgh. In 1840 he was elected Member of the Council General of the Seine.

In 1848, when the revolution of February broke out, M. Arago was appointed a Member of the Provisional Government, having been nominated to the vacant offices of Minister of War and of Marine. During the insurrection of the working classes, which occurred in the month of June 1849, he displayed great personal courage in endeavouring to appease the angry multitude, but although formerly the object of their reverence, he was no longer able to exercise any influence over them. The hopes which he had long entertained of the establishment of a Republic in France were thus completely frustrated by the folly of the people. Henceforward he ceased to occupy his attention with politics. Unfortunately about this time his health became impaired and his sight began to fail. Warned by these symptoms, he now resolved to prepare for publication the great mass of scientific writings which he allowed to accumulate during his active career.

The *coup d'état* of the 2d of December, 1852, formed the death-blow to the illusion which he had so long cherished relative to the establishment of a Republic in France. When summoned to take the oath of allegiance to the new Government, he refused to do so. On that occasion he addressed a memorable letter to the Government justifying his conduct, and setting forth his claims to the favourable consideration of his countrymen. Notwithstanding his refusal to comply with the demands of the Government, he was allowed to retain his appointment as Director of the Observatory.

In the summer of 1853 the health of M. Arago having become very feeble, he proceeded to the Eastern Pyrenees, under the care of his niece, Madame Laugier, his friends having advised him to try the effect of his native air upon his constitution. He soon afterwards, however, returned to Paris, without any hope of recovery. He died on the 2d of October, 1853, in the sixty-seventh year of

his age. His remains were interred in the cemetery of Père La Chaise. A funeral oration was pronounced over his grave by M. Flourens, the Perpetual Secretary of the Academy.

M. Arago in early life married a lady of the south of France, who has been dead many years. He had two sons by her, both of whom have survived their parents.

In his family relations he seems to have enjoyed a source of great happiness. "This man," says M. Flourens, "in whom were united so many excellencies, devoted a portion of his life to the cultivation of domestic affections. He had experienced all the sweets of filial piety; the cord of his affections extended itself without being weakened; his brothers, his sisters, were always with him, under the paternal roof; his own children and theirs were equal objects of his regard: thus he found a daughter whose pious and touching cares ought to receive this day the grateful acknowledgment of the Academy."*

Researches on subjects which have no immediate connexion with astronomy form the principal claims of this distinguished man to the remembrance of posterity; but his acute and brilliant intellect, combined with his extraordinary energy and his enthusiasm in the cause of science, exercised a powerful influence upon the cultivation of almost every branch of physical inquiry, more especially among his own countrymen. He possessed a remarkable aptitude for expounding the principles of science, and rendering them intelligible to the uninitiated classes of the community. His eloquent lectures on astronomy delivered at the Royal Observatory of Paris were always listened to with attention by a crowded audience.

M. Arago was endowed with an ardent temperament, which occasionally had the effect of involving him in controversies tending to detract from the influence so justly due to his high intellectual qualities. These, however, are faults which are more or less inseparable from human nature in its present existence. Assuredly when they have been long forgotten the name of François Arago will still continue to occupy a distinguished place in the annals of science.

It has been announced since the death of M. Arago that his writings are about to be published in twelve volumes octavo, under the superintendence of M. Barral, an intimate friend of the deceased.

Don Jose Sanchez Cerquero entered very young into the Spanish naval service: he was born about 1784. He was actively engaged during the war; but his whole leisure, from boyhood up to his appointment, in 1816, to a subordinate post in the Observatory of San Fernando, was devoted to the study and application of mathematics. Admiral Smyth, who was his comrade at the siege of Cadiz in 1810, informs us that he there commanded a gunboat under Admiral Valdez. In 1825, he was promoted to be Director of the Observatory, and immediately undertook the task of

* Madame Laugier, his niece.

bringing it up to the existing state of science. For this purpose he came to England, and made a close inspection of the Observatory at Greenwich, an account of which was found among his manuscripts; he also visited France and Belgium. In the midst of the cares which the choice and mounting of instruments threw upon him, he undertook the improvement of the *Nautical Almanac*, and the extension of its size. His first publication was on the longitude of Puerto-Rico, which appeared in De Zach's "Correspondence." In addition to the *Nautical Almanac*, he wrote on the various methods for obtaining latitude at sea by observation of altitudes, on the formulæ for reduction of observations, and on the calculation of eclipses. He published also in the Brussels Correspondence of M. Quetelet a memoir on the errors of the transit instrument, and left various memoirs on pure mathematics, &c. unpublished.

Cerquero died in 1850. He was an elegant scholar, and well acquainted with English, French, Italian, and German.

Sears Cook Walker was born at Wilmington, Massachusetts, on the 28th of March, 1805. The early life of Mr. Walker was marked by an uncommonly precocious mental developement. At the village school, while yet a child, he so easily outstripped every other boy in the school, that he became the wonder of the neighbourhood; but his very delicate bodily organisation demanded, and obtained, the most untiring watchfulness and care from his admirable mother, upon whom devolved the whole responsibility of his education, as his father died just as the son had attained the age of six years. She happily appreciated the importance of strengthening the physical, as an aid to the mental powers, and never, in the desire to have her son admired as a youthful prodigy, failed to act upon that conviction.

Her efforts, guided by excellent judgment, were successful; he was soon restored to health; and, until about a year before his death, his bodily vigour was unimpaired.

Having received preparatory education at the school of Mr. Putnam, of Andover, Mr. Walker entered Harvard College in 1821, at the age of sixteen, and during his college life it might have been doubted whether he would most distinguish himself as a linguist or as a mathematician; he could read with ease seven languages; and having a very retentive memory, could quote from them all to an extent that is rarely surpassed. He had also a great aptitude for the natural sciences,— botany, mineralogy, and geology, were his favourite recreations,— we might say his only recreations; for, in the amusements of the world, with the single exception of the Opera, he took no interest. These tastes, in addition to a fondness for chemistry, induced him to go through with a regular course of medical studies; not, however, intending at any time to pursue this as a profession.

On graduating, in 1825, Mr. Walker chose a mathematical subject for his thesis, and acquitted himself with honour. He then opened a school in the vicinity of Boston; and it was at this time

that his partiality for astronomical pursuits first developed itself, as he would often pleasantly remark, that the Observatory at Dorchester, where he was a frequent visitor, must be held responsible for all his astronomical sins. Shortly afterwards he removed to Philadelphia, and there established a school, in which he soon became distinguished for his success in imparting knowledge, and inspiring the minds of his pupils with zeal for scientific and literary pursuits. While thus engaged he was elected a member of the American Philosophical Society, the Geological Society of Pennsylvania, and of the Franklin Institute: that he proved an active and efficient member of these Societies their records amply indicate.

As soon as his means permitted, he procured a small Dollond telescope, an astronomical clock, and a 20-inch transit instrument. The use of these proved so attractive that all other recreations were abandoned, and astronomy absorbed his whole leisure. From the year 1834 to 1842, he computed the occultations visible in the United States. These were published monthly in the Journal of the Franklin Institute.

After some years of severe toil as a schoolmaster, he was, in 1836, appointed Actuary of the Pennsylvania Life Insurance Company: here he had daily occupation for only six hours, and all the remainder of his time, not required for rest, was devoted to astronomy.

In 1837 he was invited to suggest a plan for an Observatory to be erected in connexion with the Philadelphia High School; and in accordance with his recommendation, a 9-foot Fraunhofer equatoreal, a meridian circle by Ertel, having a telescope of 5 feet focal length, and a comet-seeker, were procured from Munich, and placed on suitable piers under a revolving dome in a building adjoining the schoolhouse.

The equatoreal was mounted in 1840, and here Mr. Walker laboured assiduously, in company with his brother-in-law Professor Kendall, who, as principal of the school, had charge of the Observatory, until the year 1845, when Professor Walker was called by Mr. Bancroft, then Secretary of the Navy, to a position in the National Observatory at Washington city. Here his presence was soon felt in the increased energy of the well-directed efforts of the corps of observers. It was during Mr. Walker's brief sojourn at the National Observatory that he commenced his well-known investigations in regard to the planet *Neptune*. These he continued while his health permitted, through the remaining years of his life, with increasing interest for astronomers.

While residing in Philadelphia, Mr. Walker had acquired the warm friendship of Dr. A. D. Bache, Superintendent of the United States Coast Survey, and on retirement from the National Observatory accepted Dr. Bache's offer to take charge of the astronomical department of the Survey, for the determination of the latitudes and longitudes of the principal stations of the main triangulation by astronomical observation, and subsequently of the differences of

longitude by aid of the electric telegraph. This important office Professor Walker continued to fill during the remainder of his life, to the entire satisfaction of Dr. Bache and credit of the country. It was here that he found encouragement to extend and perfect the different methods of scientific investigation which his ever-active mind was continually suggesting; its acuteness and delicacy were evinced on many occasions: we may instance his detection of a delay in the transmission of the galvanic inducing waves proportionate to the space traversed. His report to the Superintendent of the Coast Survey, under date of April 24th, 1851, is highly interesting, as it contains an account of the progress of improvement and invention in the art of determining longitudes by the electric telegraph; and proves, without the slightest undue assumption on his part, that to him more than to any other individual science is indebted for the introduction of the electric method of observing.

Mr. Walker had made the largest collection of American observations of moon-culminations and occultations ever made in this country, and had prepared to discuss them thoroughly for longitudes, and to bring them to bear, as far as applicable by the geodetic results of the Coast Survey, upon the longitude of a central point. During this discussion he reached the conclusion that the longitudes from moon-culminations could not be reconciled with those from occultations, and that the lunar theory must be re-examined for an explanation. His published reports show the successive steps of his investigation, which was not completed at the time of his decease.

An apparently slight attack of paralysis was followed by mental alienation, which continued with scarcely a lucid interval to the time of his death, which took place at the residence of his brother, Judge Walker, in Cincinnati, on the 30th of January, 1853.

This brief sketch cannot be more appropriately closed than in the language of Judge Walker:—"We may not say that he died a martyr to science, but he lived one of its most devoted lovers. His delight was among the stars,—all else was labour. This was pleasure, or rather intense delight, and he had no other passion or ambition. What he had accomplished, we know; what he might have done had his life and health been spared, it is vain to conjecture. A more simple-hearted, amiable, and affectionate person would scarcely be imagined. He wooed science with a devotion that excluded all other loves, until, in the ardour of pursuit, his brain gave way."

The following communications were made by Professor Walker to the American Philosophical Society, and published in the Transactions and Proceedings of that body:

In the Transactions.

Vol. V. On the Longitude of the Hall of the American Philosophical Society, deduced from an Occultation of *Aldebaran*, Jan. 5, 1830.

Observations on the Solar Eclipse of Nov. 30, 1834, made at Philadelphia and Germantown, Pennsylvania.

Determination of the Longitude of several stations near the northern boundary of Ohio, from Transits of the Moon and Moon-culminating Stars, observed in 1835 by A. Tolcott, Captain U. S. Engineers. Report of Committee on Solar Eclipse of May 14–15, 1836.

VIII. Researches concerning the Periodic Meteors of August and November.

Observations of Encke's Comet at the High School Observatory, Philadelphia, March and April 1842, with the Fraunhofer Equatoreal. (Walker and Kendall.)

X. Investigations which led to the Detection of the coincidence between the computed place of the Planet *Neptune*, and the observed place of a Star recorded by Lalande in May 1795. Read Feb. 19, 1847.

Proceedings.

Vol. I. On Galle's Second Comet, and reference to the Discovery by Galle of a Third Comet.

On determining Longitudes from corresponding Observations of Meteors.

The August Meteors.

Meteors of August and November 1840.

The Observations at Harvard.

The Parallax of the Star 61 *Cygni*, recently investigated by M. Bessel.

II. Periodical Meteors.

Comet of 1843.

Encke's Comet.

III. Comet of 1843.

Supplementary Letter.

IV. Comet of February 1843.

Comet in *Orion*.

Herries' Comet.

Biela's Comet.

Elements of Planet *Neptune*.

On Identity of Lalande's Star and *Neptune*.

New Elements of *Neptune*.

V. Results of his Labours in regard to the Planet *Neptune*.

Abstract of Report of the Results of Telegraphic Operations of the United States Coast Survey.

Kirkwood's Discovery of a new Analogy in relation to the Periods of Rotation of the Primary Planets.

To these papers may be added a large number of reports made by Mr. Walker, on subjects connected with the Coast Survey, and published in the annual reports of the Superintendent.

The individual—a well-known American astronomer—to whom the Council are indebted for the foregoing details respecting the life and labours of Professor Walker, concludes his communication in

the following terms:—"In my endeavour to trace the social and scientific life of my deceased friend, I have felt that it was only by those who knew him intimately, and in retirement, that the excellence of his character could be fully estimated."

Though it is not usual to introduce supplements to obituary notices contained in former reports, yet the interest which is here felt in all that relates to Francis Baily will justify the statement of some facts relative to his early life, which have been brought out by a recent examination of his correspondence, and of the journal of his voyage in America.

Perhaps the earliest mention of his name in print is in the proceedings of the American Congress. The Spanish authorities had imposed various hardships upon citizens of the United States and other foreigners, by the depreciation of their coinage; and in the discussions which took place at Washington upon this subject, the name of Baily is cited as one of the parties aggrieved. It distinctly appears that one of the objects of his tour was the formation or extension of commercial connexion, probably of some house in England. It also appears that during his voyage he gave formal notice of his intention to apply for the privileges of citizenship, with a view to take up his permanent residence in the United States; and further, that his friends in England were made cognisant of this intention. Some allusions to a young lady seem to give the reason of this contemplated change of country; but nothing is found which explains the abandonment of the plan. Two subjects are concealed in short-hand; one is that which has just been hinted at, the other is the expression of his feelings towards Washington, for whom he entertained a respect, the depth of which may best be judged of from the description of his mode of recording it.

On his return to England, he seems for some time to have had no decided plan, except that of adopting some life of active adventure. In May 1798, he was seeking a commission in the militia, and an ensigncy in the Volunteer Company of the Berkshire corps was actually offered. In December of the same year, he had been inquiring as to the means of obtaining a commission in the Engineers; and a letter from Bonnycastle, which represents the impossibility of such a thing, hints at the East India Company's Service, and informs him that several officers and other gentlemen are soon going to Turkey. In May 1799, he applied to the African Association, with an offer to enrol himself in their service as a traveller, and by a letter from Sir Joseph Banks (June 11), it appears, that, if there had been sufficient funds, his offer might have been seriously considered. In a private letter to Sir John Stepney (Sept. 18, 1799) is the following passage:—"I had proposed to myself a route which should be less circuitous than those of Park or Horneman, namely, to proceed northwardly or north-eastwardly from Calabar or Wydah, till I should strike the Niger. I had the subject so much at heart, that I would have gone through any

trials to have accomplished my object. With respect to the difficulties and dangers of such an undertaking, they would no doubt have been many; but they are things which much experience of this mode of travelling in the New World has induced me to think light of. I can even say with Horace, *Dulce periculum est.* So true is it that habit and custom can soon efface those disagreeable sensations which arise from some of the rough accidents of life. After a number of fruitless attempts to succeed in my object, and meeting with so little encouragement in the prosecution of it, I have at length determined to give it up; and an event is about to take place which most probably will prevent my ever resuming it —this is, my going to enter into partnership with an eminent stockbroker in the City."

All this time it appears that Baily was paying close attention to mathematics, astronomy, and botany. The earliest astronomical writing of his now extant is a paper (dated October 1798), written apparently for his own instruction, containing a description of a neatly drawn projection of the heavens after sunset on April 5, 1799, at which time, he says, "all the planets in our system will be above the horizon at the same time, forming a line along the ecliptic from the most westerly point to near the zenith."

The origin of the work on *Tables and Formulæ* is in a manuscript having the title "Elements of Astronomy, deduced from M. Laplace's 'Exposition du Système du Monde.'" London, 1810. This manuscript, most neatly written, was certainly intended for publication; and by being marked in pencil, "Communicated by Francis Baily, Esq.," and "25 copies for Mr. Baily," it seems to have been drawn up for some society for mutual instruction, or other private association.

To complete what was said on Baily's writings in Sir John Herschel's memoir of his life, it may be added that a large mass of his astronomical papers and correspondence, including much of the account of Flamsteed, and the whole of the Catalogue, is, or will be, deposited at Greenwich. The manuscripts of the works on leases, on interests, and on annuities, are in the library of the Institute of Actuaries. An account of his correspondence with Mr. George Barrett, which was the means of laying before the world one of the greatest improvements ever made in the calculation of life contingencies, will appear in the *Assurance Magazine* for April next.

The Council have not been furnished with any details respecting the life of Mr. DENT, but the following tribute to his merits as an artist, contained in a letter from M. Otto Struve to the Astronomer Royal, dated Jan. 17, 1854, cannot fail to be gratifying to his friends:—

"It is my fault that I have not more particularly written to you before, for the Council's Annual Report to the Astronomical Society, by what means the late Mr. Dent had contributed to advance Russian geography. Perhaps it is not yet too late to point out the following principal merits: 1st, the unequalled (at least so far as we

know) perfection of his chronometers; 2d, the liberality with which he put to our disposition a large number of his chronometers; 3d, the assistance with money, credit, and instructions, by which he enabled our Petersburg chronometer-maker, Mr. Pitt, to establish himself in St. Petersburg, on purpose that we might have our chronometers and watches cleaned and repaired *in loco* by a first-rate artist.

"The number of chronometers Mr. Dent had lent us, without any direct pecuniary remuneration,—and most of them for many years,—amounted to more than seventy. It was by the free command of that large number of his excellent productions that we were enabled to give to our chronometrical operations that rapid display, by which at present nearly the whole of European Russia is regularly covered with thousands of astronomically determined positions. We owe this acknowledgment to his memory, and would be very much gratified if you will mention it in the Annual Report."

George Frost was born on Christmas-day 1798, at Thorpe-le-Soken, in Essex, and is descended from an ancient family in Lincolnshire. He received the early part of his education at a private academy in his native village, where he studied navigation with the intention of entering the Navy. In 1819, he came to London and finished his education. He then abandoned his former intention of going to sea, and became a member of the London Mathematical Society, where he made considerable progress in that branch of science. At that time he was engaged as mathematical tutor in the family of the present Bishop of London, and in other quarters. In 1833, he was elected a Fellow of this Society, and the same year published a work under the title of "The View of the Earth and Heavens." His other publications were principally mathematical, and amongst his manuscript papers is a work on Astronomy which he was preparing for the press.

Being fond of pursuits in the country, he was latterly much engaged in directing the agricultural operations of his estates in Essex and Suffolk. He died suddenly, December 23, 1852, much respected.

Henry Harvey was born at Holt, in Somersetshire, in July 1783; he finished his education at Trinity College, Cambridge. He passed sixteen years in the East India Company's service, and was severely wounded at the battle of Assaye. Being independent in his circumstances, he returned to England, and occupied himself in his own pursuits, one of which was astronomy: in the last years of his life he took a large share in the management of a joint-stock bank. He died on the 6th of March, 1853. He was a brother-in-law of Captain Basil Hall, and an intimate friend of Captain Kater. He was three times married, and his first wife was the widow of Sir William de Lancy, who fell at Waterloo. He was a man of highly cultivated mind and manners, and much attached to science.

WILLIAM SAMUEL STRATFORD, eldest son of William Stratford, Esq., of Farncote, Gloucestershire, was born on the 22d of May, 1789, at Eltham, Surrey. He received his early education at Hanwell Heath Academy, and afterwards attended the school of a Mr. Oard, in London. He was one of a family remarkable for their mathematical attainments, and always evinced great aptness and inclination for those studies.

In the year 1806 he entered the navy as a first-class volunteer on board the Pompée, under the command of Sir Sydney Smith, whose favour he soon acquired, and from whom he invariably received the greatest kindness. He always spoke of Sir Sydney in terms of the highest regard, dwelling particularly on his friendly feeling towards, and great interest in, all the young men who came under his care. He served for some time in the Mediterranean, was engaged in the defence of Gaeta and the reduction of Capri, and served in the expedition to the Dardanelles, and at the destruction of a Turkish squadron off Prinkipos Island. He accompanied, as a midshipman, the force sent against Copenhagen, where he received a slight wound; and continued in active service till 1812. In 1815 he received his promotion as lieutenant, and after that time remained on half-pay.

He was one of the earliest and most active members of the Royal Astronomical Society, and filled the office of Secretary from 1825 to 1831. He was afterwards one of its vice-presidents, and a member of the Council until the state of his health obliged him to discontinue his attendance.

He was one of the committee of the Society to whom the state of the *Nautical Almanac* was referred by the Admiralty in 1830. In 1831 he received the appointment of Superintendent of that work, which office he held till his death, which occurred on the 29th of March, 1853. Under his superintendence the recommendations of that committee were ably carried out, the character of the *Almanac* as a work of science was greatly raised, its magnitude and circulation, and its public usefulness, were greatly increased.

Besides the performance of his public duties, he rendered other valuable services to the science of astronomy. He performed one set of the duplicate calculations for the original catalogue of the Royal Astronomical Society, for which he received their silver medal, at the same time that Baily received the gold one. In 1837 he published, as an appendix to the *Nautical Almanac*, a volume on the elements of the orbit of Halley's comet; and in 1845 completed the printing and notes, after the death of Mr. Baily, of the catalogue of stars for the British Association for the Advancement of Science. When a young man he was a correspondent of some of the mathematical periodicals. He was a fellow of the Royal Society, and was a member of, and correspondent with, several foreign scientific bodies.

Mr. Stratford will long be remembered in this Society as one of the most devoted, and for a long period one of the most active and useful, of its supporters. It should be recorded, that, during

the time he was Secretary the office of Assistant-secretary did not exist; and that the whole routine of the business was conducted by him, from the correction of the proofs of the *Memoirs* to the folding of circulars. The amount of actual labour performed by him was very considerable; and during the time in which the Treasurer did not reside in London, a large portion of the management of the accounts fell to his share. He was always willing to undertake any useful work, and brought to its performance all the energy of his character. He had strong feelings, and was accustomed to confide in their impulses, so that, as may be supposed, his career in this Society, and in the world, was not without storms. But he had great kindness of heart and good temper, and could bear with any amount of remonstrance, or even reproof, from those to whom he was sincerely attached. Among the number of these was Mr. Baily, with whom he long lived in great intimacy, and for whom he had a respect amounting to veneration.

Mr. Thomas Weddle was born November 30, 1817, at Beaumont House, near the village of Stamfordham, in the county of Northumberland. His mother's brother was the late Mr. Henry Atkinson, of Newcastle-on-Tyne, well known by his writings, especially the paper on refraction, in our Transactions.

Mr. Weddle received part of his early education at Stamfordham. He made rapid progress in classics, which however he did not prosecute to any great extent, but he subsequently acquired such a knowledge of the French and German languages as enabled him to read with facility the writings of the Continental mathematicians. The bent of his mind lay evidently towards the exact sciences, and under the care of his grandfather he acquired an accurate knowledge of the usual preliminary branches of a mathematical education. Though he was educated principally by his grandfather, he likewise received some instruction at the Anchorage School, Gateshead, of which the late Mr. James Charlton was the able conductor. Here his genius for mathematics began to develope itself, for he not only acquired a knowledge of the Elements of Euclid with wonderful rapidity, but while studying them, suggested various improvements in the demonstrations, and pointed out many extensions and modifications of different propositions. The work on geometry he studied at this school was Playfair's Euclid, in which the propositions of the Fifth Book are demonstrated algebraically. On reading the Eighth proposition of that book he remarked to his master that Playfair's demonstration of that proposition was defective: that it was not general, and that there was a case entirely unnoticed by that great geometer. While at school, Mr. Weddle became acquainted with the Cartesian method of applying algebra to geometrical investigations.

Mr. Weddle began his profession as a teacher with the Rev. Mr. Walton, of Allenheads; he shortly afterwards came to Horsley, near Ovingham, where he kept a school for some years, and where he prosecuted his mathematical studies with remarkable success.

It was while at Horsley that Mr. Weddle first attracted notice as a profound mathematician, for it was here that he discovered his *Method of Solving Numerical Equations of all Orders.* This highly meritorious and most important method was communicated to the Royal Society, and read before that learned body in 1841; it did not however appear in their Transactions, but was published by subscription in the following year, after he had removed to Newcastle-upon-Tyne. This is a method by which numerical equations of very high orders and involving large coefficients can at all times be solved, which were quite unmanageable, even by the most modern and effective processes which analytical skill had yet been able to devise. The solution of numerical equations of all orders yielded to its power: for by it Mr. Weddle obtained the actual solution, without help from logarithms or trigonometry, of an equation of the twentieth degree, complete in all its terms, and solved another of the 622d degree, consisting of four terms involving large coefficients. By this ingenious method any specified root of a given number may be extracted without a very great expenditure of labour, even to a considerable number of places of decimals.

In 1841, a vacancy having occurred in the Rev. Mr. Bruce's Academy, Newcastle-upon-Tyne, by the appointment of S. Fenwick, Esq., to a mathematical situation in the Royal Military Academy, Mr. Weddle was so highly recommended by those who knew his character and abilities, that Mr. Bruce at once engaged him as mathematical master in his distinguished seminary. Here Mr. Weddle continued about six years. On the retirement of Professor Barlow from the Royal Military Academy, at Woolwich in 1847, Mr. Weddle appeared as a candidate for the situation of a mathematical master in that institution, and though on examination he was placed first in mathematics, he was considered not the most eligible for the appointment. In the same year, 1847, he came to Wimbledon and taught the senior mathematical classes in Messrs. Stoton and Mayor's School, at that place. Here he remained upwards of two years, and when Mr. Tate was transferred from the Battersea Training College to a similar institution at Kneller Hall, Mr. Weddle was appointed his successor at Battersea, the situation having been kept open for him six months, that he might complete his term of engagement with Messrs. Stoton and Mayor. Mr. Weddle's stay at Battersea was comparatively short, not much exceeding twelve months. In 1851, he was appointed (without examination, which he refused to undergo) a mathematical professor in the Royal Military College at Sandhurst.

As a Diary correspondent, Mr. Weddle's name was recorded both in 1839 and 1840, but his first contributions to that inestimable publication appeared in 1841. As a sequel to Professor Davies' *Horæ Geometricæ*, which appeared in the Appendix to the Lady's Diary, Mr. Weddle contributed an elaborate article on the *Symmetrical Properties of Plane Triangles*, with their inscribed and escribed circles. This paper was spread over three

Appendices of the Diary, the first part appearing in 1843, the second in 1845, and the last in 1848. The subject of this paper was thought by Professor Davies to have been exhausted, or nearly so, in his last communication to the Diary, but when taken up by a master mind, it soon appeared that the properties of these figures were almost inexhaustible, and that new theorems might be discovered without limit by any one qualified for their investigation. Mr. Weddle's paper is a complete storehouse of theorems, amounting to no fewer than 249 classes of elegant and interesting formulæ.

Mr. Weddle was also one of the most valued and able correspondents of *The Mathematician*, a mathematical periodical conducted for several years by Messrs. Davies, Rutherford, and Fenwick, of the Royal Military Academy. From its commencement in November 1843, to its discontinuance in September 1850, its pages were enriched by numerous excellent papers and solutions from Mr. Weddle's pen, distinguished alike for depth and originality. In the second volume of that periodical he published his very ingenious method for the *Computation of Logarithms and Antilogarithms*, a paper of considerable interest, as by the processes there developed, with the aid of a few simple auxiliary tables, the logarithm of any number, and the number corresponding to any given logarithm, can be readily computed. A similar method of *Computing Tangents and Antitangents*, from the same pen, appeared in a subsequent number of that volume. Besides these, and many others to which allusion cannot be made here, Mr. Weddle contributed to the third volume, *A New and Easy Method of Approximating to the Unreal Roots of Trinomial Equations*, and an excellent paper *On Analytical Geometry of Three Dimensions relative to Oblique Axes.*

Of late years Mr. Weddle was a regular contributor, as far as his health permitted, to *The Cambridge and Dublin Mathematical Journal*, a work of European circulation; and his elegant papers will not suffer in comparison with those of the first mathematicians of the age, by whom that journal is supported. His very elaborate paper entitled, *On the Theorems in Space analogous to those of Pascal and Brianchon in a Plane*, is an admirable specimen of analytical skill. Availing himself of the abridged methods of notation, he deduced a great variety of interesting and curious properties relative to surfaces of the second order, with a degree of neatness and symmetry which has never been surpassed. He contributed a short paper on the *Parallelogram of Forces* to the Philosophical Magazine.

Mr. Weddle was naturally a very delicate man, and of a consumptive family. The last two or three years of his life were passed at Sandhurst in a declining state of health, indeed he latterly became so feeble that he could not walk more than a few hundred yards without feeling completely exhausted, and though the distance of the College from his residence was not more than a quarter of a mile, he felt unequal to the task of walking that

distance, having become so excessively weak as to require a carriage to convey him to and from the College. Consumption at length terminated his valuable life, on Sunday, the 4th of December, 1853, at the early age of thirty-six years. He has left a widow, but no family, to deplore his loss.

In Mr. Weddle's death, the Royal Military College lost a distinguished ornament, and mathematical science one of its most successful cultivators. Though cut off in the flower of his age, he had written enough to show what might have been expected from him had he lived. He was an excellent teacher, his own thoughts were as clear as day, and he had the tact of communicating them to others with remarkable facility. Mr. Weddle was more than a mathematician:—he was also a well-informed man, possessed of a fine taste for polite literature, with which he was well acquainted, and took delight in its cultivation; he was an acute metaphysician, and reasoned as well on other subjects as he did in his favourite science. In every relation of life he was good and amiable: an excellent husband, a generous son, and a faithful friend, so that those who knew him best will most lament his premature death.

Mr. Weddle has left several scientific manuscripts, and it is to be hoped that some one of his scientific friends will, ere long, collect the scattered emanations of his pen, and republish them, together with his manuscript papers, in a connected form. Indeed, everything that Mr. Weddle published was distinguished for originality, and it cannot be doubted that his entire mathematical papers would, if brought together, form a most attractive, interesting, and useful publication, and transmit to posterity his fame and genius as a mathematician of the first rank.

The method for the solution of equations, by which Mr. Weddle's name will be conspicuous among algebraists, proceeds neither by successive acquisition of root-figures like that of Horner, nor by continued fractions like that of Lagrange. Each correction consists in the application of an easy factor to the preceding result, and this mode of proceeding in an organised form is perfectly original. By the nature of the method, every power of the unknown quantity which is missed dispenses with a corresponding proportion of work, a facility which is totally unknown in the two great methods mentioned.

With regard to the standard Astronomical Instruments of the Royal Observatory no important or organic change has been made since the last Report of the Council in February 1853. The transit-circle still maintains its high character for steadiness, and the observations made with it are of first-rate excellence; the altazimuth has been worked with the same assiduity, and the proportion of the observations of the moon made with it to those made with the transit-circle, remains the same as in former years: finally, the accuracy of the principle of construction of the zenith tube is borne out by the observations which have been made with it.

every opportunity, though it becomes more evident that a larger object-glass, and a position for the instrument less exposed to disturbance, will ultimately be required.

The organisation of the Galvanic operations connected with the ordinary business of the Observatory is gradually becoming more complete. By means of the galvanic motor-clock and the system of wires connecting the Observatory with the South-Eastern Railroad Company's Station at London Bridge, hourly signals, giving accurately Greenwich mean solar time, are transmitted to the offices of the Electric Telegraph Company at Lothbury and the Strand in London, and to Tunbridge, Deal, and Dover, several times in the day. Signal-balls are dropped at the Strand and at Liverpool simultaneously with the drop of the Greenwich ball at 1 o'clock. In addition, by means of an ingenious contrivance at Lothbury, time-signals are sent at 10 o'clock A.M. and at 1 o'clock P.M. each day, primarily from Greenwich, to various stations on the line of the Electric Telegraph Company. Preparations are also making for the erection of a ball at the port of Deal, which it is also intended to drop simultaneously with the drop of the ball at Greenwich, by means of the wires on the line of the South-Eastern Railway.

The various difficulties which occurred from time to time in the mechanism of the barrel or smooth-motion clock, used for giving motion to the cylinder on which will ultimately be recorded the transits made with the transit-circle and altazimuth, according to the American method of self-registration, have been overcome. It now carries the cylinders put into connexion with it with perfect regularity, its rate having all desirable steadiness.

The Astronomer Royal is in negotiation with Mr. De La Rue for a supply of paper best adapted for receiving the record of the transits, and that gentleman, with his accustomed zeal for the interests of science, has given his attention to the subject. In the mean time satisfactory trials have been made of the general efficacy of the apparatus by trial of an ordinary sheet of paper applied to the cylinders, on which, by a pricker acted on at every beat of the transit-clock by the electro-magnets attached to the frame, marks were made with perfect regularity and with adequate force during several revolutions of the barrel.

From the first instant of the laying of the wires connecting the Greenwich Observatory with the stations of the South-Eastern Railway Company and the Electric Telegraph Company, it was evident that one of the earliest and most useful applications of them would be the determination of the longitudes of several of the principal observatories in the British Isles and on the Continent, which lie near the lines of the wires. During the last year, the earliest possible opportunities have been taken for settling definitively, by the admirable facilities thus afforded, the longitudes of Cambridge, Edinburgh, and Brussels, with complete success, as far as regards the galvanic communications and the observations

of the signals at all the observatories, and with only one instance of defective determination of the local time at one of the observatories.

The method was first tried successfully in May last for determining definitively the longitude of the Cambridge Observatory, the requisite preliminaries for the accurate observation of the signals and the accurate determination of local time at each station being previously arranged between the Astronomer Royal and Professor Challis, and the requisite arrangements for insuring a complete circuit between Greenwich and the Lothbury Station, and from thence to the Cambridge Railway Station, being made at the instance of the Astronomer Royal by Mr. C. V. Walker, Engineer and Superintendent of Telegraphs of the South Eastern Railway, and by Mr. Edwin Clark, Engineer of the Electric Telegraph Company, respectively. The details of the operations are given in the *Monthly Notice* for June 1853, and the definitive result deduced from 279 signals is $22^s.956$, by which the transit instrument of the Cambridge Observatory is east of the Greenwich transit-circle.

On May 25, signals conducted on a similar plan were simultaneously observed at Greenwich, and at the Terminus of the Railway at Edinburgh, for the determination of the longitude of the Royal Observatory of that place. The details of the operations were in every respect satisfactory, excepting as regards the transmission of the times observed by chronometer at the Edinburgh Station to the Observatory, by comparison with the transit-clock. Something unsatisfactory resulted in this portion of the operations, which will make it necessary to repeat the experiment. This circumstance is the less to be regretted on account of the present intention to carry the wires from the station to the Observatory on the Calton Hill, which will prevent any possibility of a like failure for the future.

But by far the most elaborate series of experiments yet made by means of the galvanic communications, is that which was made in December last for the determination of the longitude of the Royal Observatory of Brussels. Two distinct series of observations were made for this purpose, the one extending from November 25 to December 4, and the other from December 19 to 30. In the first series, Mr. Dunkin was despatched to Brussels for the purpose of observing signals and the stars necessary for determining the error of the Brussels' transit-clock, while M. Bouvy, one of the Brussels' observers, was sent to Greenwich for a similar purpose. In the latter series the observers were interchanged, M. Bouvy observing at Brussels and Mr. Dunkin at Greenwich.

In the first series the results of December 1, 2, and 4, only were available for the determination of longitude, as on those days only a sufficient number of stars were observed at both stations to give the corrections of the clocks without sensible error. The signals observed on the other evenings were, however, perfectly available for determining the average time of transmission of the

current between Greenwich and Brussels, and the retardation is proved to be very approximately equal to one-tenth of a second. The whole number of signals available for the determination of longitude on Dec. 1, 2, and 4, were 446, and the mean result for the longitude of the Brussels is $17^m\ 29^s{\cdot}256$ East.

In the second series of observations the evenings of December 24, 28, 29, and 30, were available for the determination of longitude, a sufficient number of clock stars being observed at each station, and the number of available signals observed on these evenings amounted to about 680. The Greenwich sidereal times of observation of the signals have been computed and transmitted to Brussels, but the Brussels' sidereal times have not been yet received at Greenwich; consequently, the definitive result for longitude cannot yet be given.

The cloudy weather which prevailed during the second series, and which rendered the continuance of the observations necessary for so long a period, severely tried the patience of the observers, but their perseverance has been rewarded by the most satisfactory result which has been yet obtained by this novel application of galvanism.

By a recent decree, the Observatory at Paris is entirely removed from under the superintendence of the Bureau des Longitudes. It is possible that the new administration may cause some delay in the completion of the arrangements for the connexion of the two observatories of Paris and Greenwich.

The meridian instruments of the Radcliffe Observatory have been engaged during the past year in completing the circumpolar Catalogue, on the plan of observing every star in at least *two* different years. This, though a laborious process, has been found to be not an unnecessary one for ensuring the identity of stars observed with both instruments, particularly in places where they happen to be clustered thickly together.

More remained to be done in this respect than was expected, so much so, that it was thought advisable to desist altogether, for the present, from extending the range of observations to more southern stars, for which preparations had been made. Partly owing to this circumstance the publication of the Catalogue has been delayed beyond the time expected; but it is hoped that this delay will be more than compensated for by the additional accuracy and certainty which will be attained. In the mean time, with the assistance of a series of indexes which will accompany the next volume (a great part of which is already printed), there will be little difficulty in referring to any star which has been observed. The plan of noting the magnitudes at every observation, when there is no visible impediment to a correct estimate, has led to the detection of *four* variable stars, which appears to have been hitherto unnoticed.

The positions of the stars in question are as follows:—

	Star's R.A. h m s	N.P.D. ° ′	
1	10 34 15	20 37·6	
2	12 37 32	28 6·4	
3	19 32 54	40 7·6	(2896 Groomb.)
4	23 51 1	39 25·4	

The last, it must be remembered, is the sf. of a double star. The companion is 10·5 and *not* variable.

Mr. Pogson (to whom and to Mr. Lucas, the detection of their variability is due) has watched them very carefully during the last two years, and considers that their periods and ranges of variation are nearly represented as follows:

1 period	304	days	range	(7·5) to invisibility
2 —	222·5	—	—	(7·0) to (12·0)
3 —	387	—	—	(8·0) to (13·0)
4 —	500?	—	—	(6·5) to (13·5)

Mr. Pogson considers the two first periods to be the best determined, though, of course, all will probably undergo a future modification.

Preparatory to entering on any systematic plan of research on the parallax of the fixed stars, it was thought that the Heliometer might be profitably employed in a fresh examination of those stars which have already been the subjects of observation elsewhere, as affording the best test by which astronomers may judge of its powers.

Accordingly, 1830 *Groombridge* and 61 *Cygni* were selected, and during the years 1852–53 long series of observations have been obtained of both of them. Though the whole of these observations have not yet been discussed, enough has been done to justify the announcement that, with regard to 61 *Cygni*, they fully confirm the fact of an annual parallax, very nearly of the same amount as that found by Bessel.

The stars selected for comparison with 61 *Cygni* were different from those Bessel used; one being 7320 B.A.C., nearly of the 6th magnitude, and the other 41030 Lalande, of the 8th magnitude. Both stars lie nearly in the direction of the components of 61 *Cygni*; the former (7320 B.A.C.) being about 2090″, *in arc*, distant from their centre, and 41030 Lalande about 1480″. The principal reason for choosing them was that they were both of sufficient brightness to allow of being brought into juxtaposition with 61 *Cygni* without reducing the aperture of either segment.

The observations already reduced, comprehending nearly three periods of maximum and minimum, treated in the same way as Bessel treated his, give the parallax of 61 *Cygni*,

By 7320 B.A.C. = +0″·364 Mean Error ±0″·115
41030 Lalande = +0·400 ±0·078

The mean of which is +0″·384, mean error ±0″·067

last determination, it will be remembered, was 0″·348 ± 0″·014, subject to a further correction for the effect of temperature on the micrometer-screw, which Dr. Peters considers (in an able paper in *Ast. Nach.* 866, p. 38), would increase it to 0″·360, mean error ±0″·016.

With regard to 1830 Groombridge, the result promises to be less decisive; but the present is not an occasion for entering into a discussion of this subject, and there is less necessity for doing so, as the observations themselves, and the deductions from them, will be in the hands of astronomers in the course of a few months.

The electric communication between this Observatory and the Great Western Railway Station, whence a direct correspondence may be carried on with the Royal Observatory, which was mentioned as being in contemplation last year, was completed several months ago, but no experiments have been yet made with it.

The report of the astronomer at Cambridge to the Observatory Syndicate was mentioned in the *Monthly Notices*, vol. xiv. p. 25. The meridian observations of 1848 are now printed, and vol. xvii., containing those of 1846, 1847, 1848, is out of the press, and will soon be distributed. Professor Challis is now proceeding with the reduction of the meridian observations for 1849 and following years. In this labour much time is saved by making use of the instrument described in vol. x. p. 182 of the *Monthly Notices*. The publication of the equatoreal observations of planets and comets is in a more hopeful state than formerly. A scheme has been devised, applicable to all the observations as yet unpublished in a regular manner, according to which the record of each observation will be completely exhibited, and the mean result of the series of observations of any body on one night may be readily obtained. The calculations of each comparison of a planet or comet with a star, will not be so full as in the scheme originally proposed, but the mean result will suffer no diminution of accuracy. The material of approximate reductions for immediate use, on which much time has hitherto been expended, will, for the future, be made available in the final reductions for publication. The observations of the asteroids, particularly with the Northumberland equatoreal, are diligently pursued, to keep this numerous class within view, and under the control of observation.

The observations now making at the Edinburgh Observatory are chiefly confined to meridian measures of stars, and are completely reduced up to the end of 1852, and partly to the end of 1853. The printing, which had been delayed on account of official difficulties, has been resumed, and a slight alteration has been introduced on the excellent method commenced by the late Mr. Henderson, but of a nature to carry out his views more completely, viz., to insert more of the particulars of the observations themselves as made and entered in the note-books at the time, so as to enable persons at a distance to have a complete knowledge of the exact value of the

raw numbers to which the subsequent computations have been applied.

A long-proposed scheme of further utilising the scientific observations, has lately been realised in the establishment of a time-ball on the lofty tower of Nelson's Monument, in the neighbourhood of the Observatory, and in immediate connexion with it by electric wires, through which the signal is given at once from the transit-clock. During the three months that the machine has been in operation, it has been found of such extensive benefit, and of such extreme accuracy, that there are movements commencing to erect time-balls in Glasgow, Greenock, Dundee, and other Scottish ports; and the ball being raised by local means, to have them all dropped by the electric signal from the Edinburgh Observatory.

An attempt to determine the difference of longitude between Greenwich and Edinburgh in the course of last summer, was not so successful as it might have been, owing to a change of rate in the portable time-piece employed in communicating between the Observatory and the Edinburgh telegraph station, where the wires from Greenwich terminated. Means have, however, been lately supplied by Government for bringing the line of wire up to the Observatory itself; and some further points essential to developing the uses of the establishment are at present under consideration in the same quarter.

At Durham the chief attention has been directed to observation of the small planets and computations relating to them. Mr. Ellis, the observer, was placed upon the establishment at Greenwich in the early part of the summer, and has been succeeded at Durham by Mr. George Rümker, whose name is well known as a practised computer.

The account given of the Liverpool Observatory in the Report of last year will serve for the present year almost without alteration. Mr. Hartnup continues to devote his principal attention to those extra-meridional observations of a delicate nature, which are also of pressing and immediate interest. The pages of the *Astronomische Nachrichten* will best show the industry and skill of this zealous observer, the quality of his instruments, and the European estimation in which the Liverpool Observations are held. In computing the orbits of *Calliope*, *Thalia*, and *Euterpe*, MM. Bruhns and Marth have relied very much on Mr. Hartnup's places. Mr. Hartnup observed Comet III. 1853, *in daylight*,—a remarkable phenomenon, only seen by M. Julius Schmidt and himself, so far as we have heard.

The meteorological department of this Observatory has been carefully attended to, and the results made public. An account is given every week in the Liverpool *Times* of the *general* results, and another, in a more precise and tabular form, in the Liverpool *Mercury;* the latter is identical with the statement furnished to the Liverpool Medical Office of Health, agreeably to the requisition of the

Health Committee. A summary of these results for the two years ending Dec. 31, 1852, was presented this year to the Literary and Philosophical Society of Liverpool, and has been published in their Proceedings.

These purely scientific researches do not, however, constitute Mr. Hartnup's main business, that of assisting, with all his might, in improving the aids to navigation. The Society will be glad to learn that seamen are beginning to take somewhat more interest in matters of such vital interest to them personally, and which nationally are so important. Attention has frequently been called to the great advantages of the mode of rating chronometers pursued at the Liverpool Observatory, in which a correction to the rate is given *depending on the temperature*. It is found, experimentally, that when a captain will apply the rate thus corrected, the performance of all chronometers is much improved, and in many cases the change is wonderfully for the better. Yet it is only very gradually, and among the more intelligent of foreign captains (especially Americans), that this simple precaution can be introduced. In testing the requisite qualifications of a mariner, it might be advisable to call his attention to the fact of the general imperfection of chronometers, and the mode of correcting it empirically; at any rate, it is not too much to expect that he should be able to note a thermometer, and take out a corresponding rate from a table furnished to him. In steam navigation, the danger from an incorrect rate is quite of another order from what it used to be in sailing vessels.

The time-ball, which is dropped at the Liverpool Observatory at 1 o'clock, Greenwich Mean Time, would be of more service if it were in a more conspicuous situation; and it has been proposed to drop the ball from some more elevated point. Whether the ball be dropped from Greenwich or from Liverpool is not of much practical importance, but it must be dropped, *directly*, from one of the two Observatories. At present, perhaps, few seamen would pay attention to this easy mode of getting a *rate* and an *error*, quite independently for themselves, as a check to those furnished by the rater; but the observation takes so little time and costs so little trouble, that a little persuasion might be sufficient to induce its adoption. There are risks at sea against which no foresight can provide; but losses from defective compasses or from ill-regulated chronometers should be treated as crimes, since common care and common sense will secure the efficacy of both these instruments. It is to be feared that life and property to a large amount are yearly sacrificed for want of a little elementary knowledge and a small amount of precaution on the part of our seamen, who neglect the safeguards which are furnished them by modern science.

Captain Jacob, of the Madras Observatory, has forwarded to the Society, and to most of our active members, copies of a new catalogue of 144 double stars. This is founded on observations made

with the Lorebours equatoreal in 1850–2, and may be considered to be a continuation of the Poona Catalogue by the same observer, published in the 17th volume of our Transactions. The means of each set of observations, with remarks, is first given (there are 501 sets), and then follows the Catalogue itself, in the usual form. The stars, which are suspected of proper motion, are set down for two or more dates. The most remarkable among them is *α Centauri*, in which the change of angle is very conspicuous, even in this short period. There is no need of recommending the Poona and Madras Catalogues to the attention of double-star observers, as Captain Jacob's zeal and skill are sufficiently estimated, and this important contribution to one of the most interesting branches of practical astronomy is sure to be rated at its intrinsic value. We regret to learn that the climate of Madras does not agree with the astronomer, and that it is not particularly favourable for astronomical observations.

Since the last Annual Report Mr. Sheepshanks has been engaged in examining those Standard Yards which presented any anomaly in his previous observations. The method of floating the bars in mercury (which seems to be almost perfect, but is very troublesome) assumes, that the bars are uniform in their composition and alike in form. It would appear that in some instances uniformity of composition has not been attained, and that the bars do not float even, and the divisions are, consequently, not in good focus. Mr. William Simms, who always prepared the bars for observation, made a note of these unsymmetrical bars, and Mr. Sheepshanks has recently re-compared them. In these later observations, the bars rested on a block and roller or stirrup, placed at the distance assigned, theoretically, by the Astronomer Royal. In two bars the divisions were found to be unsatisfactory, the coaling having been carried too far, and the edges of the divisions rounded. These will be redivided and measured when the temperature becomes nearly 62°.

The copper, brass, cast-iron, Low Moor and Swedish bars, which had been carefully compared in cold weather with the bronze standard, have been again carefully compared during the past summer at the highest natural heat, and also with the temperature artificially raised. These observations have been reduced at Greenwich, under the direction of the Astronomer Royal, and they supply an excellent scale of *comparative* expansions of the different metals from about 45° to 65°. This range is sufficient for what is at present required; but it is intended to carry the comparative measures much higher (and perhaps lower), and also to get an *absolute* measure of expansion. It is known that the usual tables of expansion are very erroneous, especially in assuming that the rate is uniform between freezing and boiling; whereas the expansion for 1° at 212° is very much larger than that for 1° at 32°. As the bars in the comparisons above mentioned were plunged in the same mercurial bath, and the temperature very carefully ascer-

tained by plunged thermometers divided by Mr. Sheepshanks himself, it is hoped that these determinations possess very great accuracy. The expansion of cast-iron is lowest, and that of cast-steel next in order. The observations for restoring the lost parliamentary standard (that is, a new type of the *optical* yard with numerous authentic copies) may be considered therefore to be all but finished. Some time and trouble are necessary for reducing all the observations to order, but there is very little work to be done and no difficulties.

Some attempts have been made to ascertain the causes of the anomalies which are to be found in the observations of MM. Baily, Murphy, &c. in 1836, but only with partial success. Mr. Sheepshanks found, as he expected, that when two bars are compared with the Society's apparatus (employed by those gentlemen), the difference varied according as the bars were on the nearer or farther T piece: the bar nearest the observer appearing longer, and that farthest from the observer appearing shorter, than it should do. This difference, however, though always the same way, changed considerably in magnitude; and when, in consequence of the cold weather, the to-and-fro motion became stiffer, the irregularity was almost lawless.

An attempt to measure a brass bar belonging to the Kew Observatory was quite unsatisfactory while using the Society's apparatus. The anomaly depending on position was very large, and the final means contradictory. It is almost certain that these errors were not caused by temperature, or by the insecure mounting of the microscopes; but were probably a compound of flexure, racking of the frame, and clinging of the Kew flat bar to the T piece.*

Mr. Sheepshanks conceives that the Society's apparatus could be greatly improved by some slight alterations; and, perhaps, it would then serve its purpose, if delicately and judiciously handled. It is very convenient and handy, and if it could be relied on, would serve for the graduation of scales, as well as for the comparison of different measures.

The scale of the Society is likely, we believe, to be ultimately placed in a national collection, and if the Society is willing, and the Standard Committee approves of his proposal, Mr. Sheepshanks intends to replace it with another scale of the same form, but shorter, which will present a yard *decimally* divided. This, he conceives, would be the most useful scale which could be made for the Society's objects. At the same time such alterations should be made in the measuring apparatus as will fit it for service.

The preparations for executing the *touch* or *end-yard* are almost completed, and the types or generators nearly made. The subject has been very carefully considered, and at present no difficulties are foreseen. The ends are to be ground on a flat surface, the centre of the bar being made the centre of motion. The

* Subsequent comparisons of the Kew scale with the Government apparatus at Somerset House, were fairly consistent; the Kew bar was supported on numerous rollers.

grinding apparatus is made, and looks promising enough. The touching surfaces in the standard bars are to be of agate, or some similar material, expanded into the axis of the bars. It is intended to prepare standards of different metals, so that future experimenters may not be teased with large differences of expansion.

Several methods of comparing two touch-bars have been suggested, some of which promise to succeed. Possibly, it may be found that the most accurate method will not be too nice for ordinary purposes, but, at present, the subject has not been sufficiently examined. There is no reason to doubt but that a satisfactory practical result will be attained, and that without much delay.

In a report presented, or shortly to be presented, to the Government, the Committee for constructing the standard yard and standard pound has expressed its opinion, in the strongest manner, that the guardianship, use, and regulation of the weights and measures of the empire should be given in future to a man of science, specially selected for his fitness for the office. There is, perhaps, no reason to fear the scandalous incompetency which distinguished the former keepers of these important types, to which, ultimately, most material things must be referred; but even a careful, and respectable, and conscientious man will not do, unless by education and natural talent he is fitted for the duty. A good geometer, with delicate eyes and fingers, well acquainted with cognate subjects, and familiar with the languages and practices of the Continent, would do himself honour and be of great service to the country in such a post, and it is to be hoped that no one less qualified will be appointed. The desire which the chiefs of our Government have shown, especially in later times, to select well qualified men for their scientific appointments, is a warrant that proper representations on these points will be attended to if judiciously urged.

The committee of the Royal Society for promoting the erection of a large reflecting telescope in the Southern hemisphere, have applied to the Government for the necessary funds; and it is understood that their application has been favourably received. The statement of the sum required was based upon an estimate furnished by Mr. Grubb of Dublin, on the supposition that the telescope is to be a 4-foot reflector, mounted equatoreally, on a plan arranged by Dr. Robinson and Mr. Grubb. It must, we apprehend, be understood that this plan is only provisional, as no decision, we believe, has yet been arrived at, on the mode of overcoming the difficulties which are peculiar to the equatoreal movement of a reflecting telescope. The Society will learn with satisfaction that the more immediate management of this business is intrusted to a sub-committee, consisting (besides Dr. Robinson), of the Earl of Rosse, Mr. De La Rue, and Mr. Lassell; and the experience and judgment of these four gentlemen, both in optical and in engineering arrangements, are a sufficient guarantee to the astronomical world that no construction will be distinctly recom-

mended for adoption until all the difficulties incidental to it have been sufficiently considered.

Four small planets revolving between the orbits of *Mars* and *Jupiter* have been discovered since our last anniversary; the entire number of bodies in this remarkable group now amounts to 27.

Two out of the four new planets were detected during the same night (April 6); one early in the evening by Dr. Annibal de Gasparis, at Naples, and the other some hours later by M. Chacornac, of Marseilles, who is understood to be engaged in the preparation of a series of charts under the superintendence of M. Valz. The former has been named *Themis*, the latter *Phocea*. *Themis* was a very minute object, estimated as not brighter than stars of the twelfth magnitude at the time of its discovery, and the greater merit therefore belongs to the diligent observer through whose exertions we have been made aware of its existence. The period of *Themis* is about 2044 days, and that of *Phocea* 1352 days.

The third planet of 1853 was found by Dr. Luther, of the observatory at Bilk near Dusseldorf, on the 5th of May, apparently during a comparison of one of Mr. Bishop's ecliptical charts with the heavens. At the suggestion of Baron von Humboldt, this planet has received the name *Proserpine*. Its revolution is accomplished in about 1580 days.

The latest planetary discovery was made by Mr. Hind, at Mr. Bishop's Observatory, on the evening of the 8th of November, when an object shining as a star of the 9th magnitude upon the ecliptical map for 3^h R.A., was speedily proved to be a new planet, which it is proposed to call *Euterpe*. The position on the above night was less than 1° distant from the point at which *Thalia* was detected eleven months previously. So far as can be ascertained at present, the period of *Euterpe* would appear to be 1313 days, or about a fortnight less than that of *Vesta*.

Five comets have been discovered during the past twelvemonth; two out of this number have become unusually conspicuous in the heavens.

The *first* was found, independently, by our Associate Professor Secchi, at Rome, on March 6; by M. Schweizer, at Moscow, and Mr. Tuttle, at Cambridge, Massachusetts, on the 8th; and by Dr. Hartwig, at Leipsic, on the 10th. It was not visible to the naked eye. The elements bear a strong resemblance to those of the comet of 1664, about which so much has been written by Lubienietski and others, but the parabola appears to satisfy the observations so well as to decide against the identity of the two bodies. A direct calculation assigns a period of 1200 years.

The *second* comet was discovered by M. Schweizer, at Moscow, on the 4th of April, and continued visible in Europe before the perihelion passage for about three weeks. At the end of the month it was detected at various points in the southern hemisphere; the nucleus had the brilliancy of stars of the 3d magnitude, and the

tail was 5° or 6° in length. Various observations of this comet, taken at sea, have appeared in our *Monthly Notices*. The orbit exhibits no perceptible deviation from the parabola.

The *third* comet, called by some "the great comet of 1853," was discovered by M. Klinkerfues, at Göttingen, on the 10th of June, at which time it was a somewhat faint telescopic object. The perihelion passage took place on the 1st of September, and about a fortnight before this date the comet had become sufficiently bright to be seen without optical power. During the last ten days of August it attracted general attention in the western heavens, by the brilliancy of its nucleus and the length of its tail. When finally sinking below our horizon, a few days previous to perihelion, the head was brighter than stars of the first magnitude. The tail is reported to have extended from 15° to 20° from the nucleus, as viewed in the clear sky of Italy and more southern stations; in this country it was not traced more than 8° or 10°. A remarkable series of observations of this comet was obtained in full daylight by M. Schmidt, at Olmütz; though distant but a few degrees from the sun's place, it was seen and well observed each day from the 30th of August to the 4th of September inclusive. Mr. Hartnup succeeded in finding the comet in daylight on the 3d; his interesting remarks thereupon will be found in the *Monthly Notice* for November. The observations of this body do not exhibit any marked indications of ellipticity. The elements have some resemblance to those of a comet observed by Tycho Brahe in 1582.

The *fourth* comet was discovered by Mr. C. Bruhns, of Berlin, on the night of September 11, soon after which date it was visible, though not very conspicuous, to the naked eye. The elements have some resemblance to those of the comet observed in the year 1582 by Tycho Brahe, and computed by Pingré, but the parabola represents the observations very closely. The comet was visible both before and after perihelion passage.

The *fifth* comet was discovered by Mr. Van Arsdale, at Newark, New Jersey, U.S., on the 25th of November, and on the 2d of the following month by M. Klinkerfues, at Göttingen. It is still visible, but continues to be, as at the epoch of discovery, a faint telescopic object. The perihelion distance is very considerable, and only exceeded in a few previous comets.

MM. Encke and Hansen, to whose labours on the subject of planetary perturbation astronomy already owes so much, have recently been engaged in researches on the theory of the movements of the minor planets, and have both arrived at apparently very satisfactory results. Each of these geometers has assigned formulæ by which it would appear to be possible, without an immoderate amount of labour, to calculate the perturbations of any planet whatever throughout an indefinite period of time, whether past or future. Dr. Brünnow, who has co-operated with M. Encke in his researches on this subject has calculated from such formulæ the perturbations of *Flora* by *Jupiter* and *Saturn*, employing the co-ordinates of the disturbed planet as on the occasion of

labours relative to the calculation of the perturbations by quadratures. M. Hansen has exhibited a similar application of his method by calculating from his formulæ the perturbations of *Egeria* by *Jupiter*.

The recent researches of Mr. Adams in lunar astronomy have led to some important results. It appears from a searching examination of Burckhardt's Lunar Tables by that eminent geometer, that the fundamental expression upon which the tables of the parallax are based has been seriously vitiated, in consequence of several of the terms being affected with errors of considerable magnitude. Mr. Adams has found that the error in the moon's computed place resulting from the combined influence of these errors in the fundamental expression may occasionally amount to between 6″ and 7″. This imperfection of Burckhardt's tables was the more difficult of detection, from the artificial manner in which their author has presented Laplace's theory, upon which his tables of the parallax are professedly founded; but it acquires great importance from the circumstance of Burckhardt's Lunar Tables having been chiefly employed in recent years in computing the moon's place for the Ephemerides which are published annually in the different countries of Europe. The rectification indicated by Mr. Adams has already been taken into account in the computations for the Nautical Almanac, the Berlin Ephemeris, and doubtless all other works of the same kind, in the preparation of which it has been usual to employ Burckhardt's Lunar Tables.

Mr. Adams has also subjected to a rigorous scrutiny the expressions for the moon's parallax which Damoiseau, Plana, and Pontécoulant, had severally deduced from theory, and has succeeded in rectifying the labours of these geometers so effectually that the three expressions for the parallax now exhibit an agreement with each other which leaves nothing further to be desired. Finally, he has shown that Henderson's value of the constant of parallax, as deduced from observation by the aid of Damoiseau's Lunar Tables, when slightly amended, exhibits a perfect accordance with the corresponding result deducible from theory by the aid of M. Peters' value of the constant of nutation.

The results to which Mr. Adams has been conducted by his researches on the moon's parallax admit of being directly verified by actual observations of the moon in our own day. The case is different with respect to another department of the lunar theory which has recently engaged the attention of that geometer, and to which his powers of analytical investigation have been successfully directed. The secular inequalities of the moon's motion cannot fail to be always regarded with deep interest, not only on account of the important chapter which the researches relative to them form in the history of the theory of gravitation, but also by reason of the valuable light which they are calculated to throw upon ancient chronology. Mr. Adams has recently detected the existence of a class of terms in the expression for the secular inequality of the moon's mean motion, which had escaped the sagacity of Laplace, and which, from the considerable magnitude of

their aggregate effect, may not impossibly exercise a sensible influence on some of the ancient eclipses. This question can only be definitively decided after Mr. Adams has subjected the secular inequalities of the perigee and node of the lunar orbit to a similar process of investigation, an object to which it is understood that his attention is at present directed.

An important investigation of the theory of the pendulum, taking into account the rotation of the earth, has recently been published by the Physical Society of Danzic, being a memoir on the subject by M. Hansen, which has been honoured with the prize of the Society. The chief novelty of the investigation consists in introducing the supposition of the pendulum being not a mere mathematical point, but a physical agglomeration of particles. By adopting this more general view of the subject, M. Hansen has succeeded in deducing several results of a hidden character which had hitherto escaped notice. The most important of these consists in the fact that a rotatory motion of the pendulum about its axis is capable of exercising a very sensible influence on the azimuthal motion of the plane of oscillation. M. Hansen illustrates his results by a variety of striking examples, and he concludes his valuable essay by investigating the motion of a pendulum of a novel construction invented by himself, with the view of obviating certain disadvantages attending the usual form.

The results of Mr. Lassell's Maltese expedition are mentioned in the *Monthly Notices* for November and December 1852, and for March and April 1853, and the observations of *Saturn* are now in course of printing in the *Memoirs*. Some observations on the great nebula of *Orion* and some other nebulæ, are now in the hands of the Secretaries. Nevertheless, Fellows would feel that this report was incomplete if it omitted some mention of so remarkable an undertaking. Mr. Lassell's principal inducement was a desire to take advantage of the approach to conjunction of the remotest three planets, and to observe them in a lower latitude. Malta, as a British possession easily accessible, and known to be favoured with a large proportion of clear sky, appeared to unite more advantages than any other place. It was hoped that its position, surrounded by at least a hundred miles of sea in all directions, would secure an equable temperature and a quiet atmosphere. Nor was this hope disappointed, the tranquillity of the air being even more conspicuous than its transparency. The following extract of a letter from Mr. Lassell to one of the Secretaries may be quoted: "Excepting the transparency of the obscure ring of *Saturn*, perhaps my discoveries abroad were rather negative than positive. I ascertained, at least to my own conviction, that no other satellite exists about *Neptune* large enough to give hope of discovery without considerable improvement of our telescopes. Also, that while I was enabled most fully to confirm my discovery of the previous year, of two new and more interior satellites of *Uranus*, I arrived at an equally strong conviction that these two,

together with the first two satellites simultaneously discovered by Sir W. Herschel in 1787, constitute the whole of the planet's retinue hitherto discovered. In the nebula of *Orion* I have, I believe, seen some minute stars in the neighbourhood of the trapezium which are new. On the other hand, some of Mr. Bond's stars I have not been able to make out. A comparison of Sir John Herschel's, Mr. Bond's, and my own drawings of this wonderful object must, I think, suggest the idea of change in the nebula, or variability of the stars, or otherwise, a less uniformity of delineation of the same thing than might have been hoped for."

The Fellows will most assuredly join the Council in congratulating Mr. Lassell on his safe return, and in expressing their high sense of his devotion to astronomy. Nor will it be forgotten that such expeditions are sure to leave something behind, as well as to bring something home. Many a year from this date, the obituary notice of some distant correspondents from Malta, or from the Cape, may record that their attention was first called to astronomy by Lassell or Herschel.

Mr. De La Rue has recently presented to the Society a beautiful coloured representation of *Saturn*, embodying *exclusively* the results of his own observations of the appearance of the planet during the latter part of the year 1852. In this drawing, which is highly creditable to the observer and to his instrument (a 13-inch Newtonian of 10 feet focal length, mounted equatoreally, and constructed by himself), several interesting features are exhibited, most of which have been already brought under the notice of the Society. It may be mentioned that the outer ring exhibits the division seen by Mr. Dawes and other observers, and also a bright zone close to it, which does not appear on the drawings of other observers of the planet. As Mr. De La Rue has engraved the drawing on steel, and very liberally placed copies of it at the disposal of Fellows of the Society, those who are not already in possession of one can obtain a copy on applying to the Assistant-secretary.

The beautiful art of Photography seems likely to be of much utility in conducting to a more accurate knowledge of the physical constitution of the celestial bodies. At the Annual Visitation of the Royal Observatory of Greenwich, in the month of June last, much interest was excited by the exhibition of a photographic image of the moon in her first quarter, which had been taken with the great refracting telescope of the Cambridge Observatory, U. S. At the meeting of the British Association held in the month of September, Professor Phillips exhibited several interesting specimens of the same kind taken with a telescope of eleven-foot focal length. Mr. De La Rue, who has also turned his attention to this department of photography, exhibited to the Society in the course of the past year an apparatus which he has contrived to facilitate the taking of such images. A good collodion picture of the moon was shown which had been taken by him in 30 seconds by the aid *of this* apparatus.

Our Transatlantic brethren continue to cultivate the various branches of astronomy with the same persevering energy which has already secured for them so honourable a position in the scientific world. Their labours during the past year have been signalised by the publication of what is intended to form the first volume of an Astronomical Ephemeris, a work resembling in its main features the *Nautical Almanac* and other similar Ephemerides which are published annually in the principal countries of Europe. A brief account of this important production has already appeared in the *Monthly Notices*. It is hardly necessary to state that the expense of publication is defrayed by the Government of the United States, which has on several former occasions exhibited a gratifying proof of its enlightened zeal in promoting the advancement of astronomical science. Lieut. Davis of the United States Navy has been appointed to the general superintendence of the Ephemeris; the arrangements connected with the theoretical department have been confided to Professor Peirce. The cordiality and zeal with which various individuals of acknowledged competence, residing in different parts of the Union, cooperated in executing a large portion of the laborious calculations required for this work, cannot be too strongly applauded by every lover of science; while such spontaneous efforts constitute as favourable an omen as could be desired of the future career of eminence which, in all human probability, is in store for American astronomy.

The Council can only briefly refer to the American Lunar Tables which have been recently published in connexion with the Ephemeris above mentioned. These tables are founded on Plana's theory of the moon, modified by the recent researches of Airy, Hansen, and Longstreth. The artificial mode of forming the arguments, which had been employed by Mayer, Bürg, and Burckhardt, is rejected, the expressions for the co-ordinates of the moon's place having the same form which they assume when deduced directly from theory, subject to a slight modification of the expression for the latitude which facilitates the process of computation, while it leaves the connexion with theory still obvious. But the peculiarity by which these tables differ from all other lunar tables, consists in their being so constructed as to give the values of the arguments in *time* instead of *arcs of the circle*. This mode of construction, which had been already employed successfully by Carlini in his solar tables, doubtless affords great facility in forming the arguments; but when the question refers to the theory of the moon's motion, which is a vastly more extensive subject, it would be premature to pronounce an opinion on its merits without some actual experience of its working.

With respect to several other investigations which the publication of the American Ephemeris has given rise to on the part of American astronomers, the Council can only allude to a determination of the sun's semi-diameter by Professor Winlock, from observations made at Greenwich by Bradley and Maskelyne with Bird's mural quadrant, and from the Greenwich observations made with

the mural circles between the years 1836 and 1851 under the superintendence of the present Astronomer Royal. The anomalous character of the results derived from the modern observations induced Professor Winlock to scrutinise the data more closely, when it appeared probable, from a comparison of the determinations of the different observers collected together into separate groups, that the inconsistencies were attributable to some cause which was constant for the same observer, but which operated differently with respect to different observers. This conclusion agrees with the result of an investigation of the sun's horizontal diameter, which had been undertaken about the same time by M. Goujon, a French astronomer, and which was founded upon observations of the transits of the opposite limbs of the sun made in recent years at the Observatories of Paris and Greenwich. M. Goujon found that the time occupied by the transit of the sun's disk over the meridian was not sensibly affected by the use of diaphragms of different shapes and sizes, whence it was manifest that the inconsistencies which presented themselves when the mean results of the different observers were compared together, as regards both the Observatories above mentioned, could not be attributable to diffraction. If the discordance is due to irradiation, which is exceedingly probable, it would seem to confirm the theory of this principle originally suggested by Galileo, viz., that it is a physiological effect which is liable to be modified by the peculiar constitution of the eye of the observer.

The Council cannot omit to make mention of a remarkable work by M. Struve, which was published in 1852, but of which a copy did not reach England soon enough to be commented upon in the Annual Report of the last year. The work in question is in some measure a summary of the sidereal labours of the great Russian astronomer during his residence at Dorpat, and gives the mean places of all the stars (the greater number of which were double stars) observed under his own direction, and that of his successor, M. Mädler, from the year 1822 to 1843. It must also be considered as complementary to the *Mensuræ Micrometricæ*, published in 1837, and both volumes taken together constitute one of the greatest boons conferred upon sidereal astronomy in the present century, the first volume containing the distances and positions of upwards of 2500 double stars, and the other, accurate mean places, for 1830, of all these, as well as of the others more recently discovered.

An elaborate abstract of the introduction to the catalogue, and a general account of the contents of the work, are given in the *Monthly Notice* for January, recently distributed; and it will be sufficient to draw the attention of the members to this, to enable them to gain a competent knowledge of its very great importance. We may, in the mean time, content ourselves with pointing out that almost all the fundamental quantities required in the star reductions, including the nutation, precession, aberration, and the

constant of refraction, are deduced independently from the observations themselves; that the proper motions of all the stars of the catalogue which are found in the *Fundamenta* are accurately calculated; and finally, that the catalogue itself will be, for the astronomers of the next generation, the great treasury from whence the materials will be drawn for making the first great step in the hitherto neglected theory of the proper motions of telescopic stars.

Some interesting researches on the theory of the double star η *Coronæ* have been communicated to the Society in the course of the past year by M. Villarceau. In a previous investigation that geometer had shown that, from the close physical resemblance of the principal star and the companion, and the consequent impossibility of distinguishing the one from the other in the early observations of Sir William Herschel, the period of revolution was affected with an ambiguity, being either 43 or 67 years, according to the mode of interpreting the observations of the last-mentioned astronomer. M. Villarceau announced on that occasion that, in the year 1853, the observations executed by modern astronomers would admit of the two orbits being sufficiently separated to decide the question of the true period of revolution. The period of 43 years was that which had hitherto been generally recognised as belonging to the star; but M. Villarceau seemed rather disposed to believe that the true period of revolution was 67 years. The result of his recent investigation of the subject has afforded a complete confirmation of his views, the latter period representing the totality of the observations in a most satisfactory manner.

It will be remembered that the last Annual Report of the Council contained an account of a remarkable paper by M. Wichmann on the parallax of the star *Groombridge* 1830. By a combination of his own measures of distance and those of Schlüter, of this star from three other stars, he arrived at a result startling and unexpected in itself, and contradictory to those arrived at separately by M. Peters and M. Otto Struve.

The result arrived at was this, that the principal star *Groombridge* 1830 has a conspicuous parallax amounting to $0''{\cdot}71$, but that one of the comparison stars denoted by (a) has also a parallax still more remarkable, amounting to $1''{\cdot}17$. On the contrary, M. Peters, by observations made at Pulkowa with Ertel's circle, had obtained for the parallax of *Groombridge* 1830, $0''{\cdot}226$; and Otto Struve, by micrometer measures of polar distance made with the great refractor, had obtained a result less than $0''{\cdot}1$. Finally, M. Wichmann's own discussion of the measures of Schlüter gave $0''{\cdot}18$.

Consequently, though there was nothing absolutely incompatible with known facts regarding the parallaxes of the stars in general in M. Wichmann's result, yet, since it was decidedly contrary to all preconceived notions, that stars so low in the scale of magnitude as (a) should have so large a parallax, and the result, as regarding the principal star, was at variance with the investigations of other astronomers, it was necessary to look most jealously upon the more

questionable parts of the investigation, and especially upon those assumptions by which the large parallaxes in question had been obtained. These views were distinctly stated in the account of M. Wichmann's paper inserted in the last Annual Report; and the paper itself has been since that time subjected to a most searching analysis by M. Peters, in a paper given in Nos. 865–6 of the *Astronomische Nachrichten*, of which a lucid abstract has been made by Mr. Grant, and printed in the number of the *Monthly Notices* for December 1853.

M. Peters observes, as had been previously observed in the Report of the Council before alluded to, that the sole ground on which the large parallaxes in question were attributed to the stars A and (a), is the precarious assumption that the difference in the summer and winter measures of a distance of more than a degree of arc by the heliometer, which amounted to about 2″, was to be attributed altogether to parallactic effect, and not to an error in Bessel's temperature-correction applied to the value of the screw of the micrometer.

In defence of this view of the subject, M. Wichmann alleges, first, that the measures can be reconciled on the supposition of parallax whose maxima values occur in summer and winter; and, secondly, that if the effect be attributed to temperature, the coefficient would result five times as large as that deduced by Bessel. To this reasoning M. Peters objects that if, of the measures instituted by Bessel for determination of the temperature-correction, those of Schlüter alone were used, the result would be nearly four times as great as that given by Bessel, and that the effect, which varied with the season of the year, could not be due to parallax, since, "although the stars of comparison lay in every direction round the principal one, the variations for each day of observation always took place in the same direction."

On the whole, therefore, M. Peters is inclined to reject the resulting parallaxes deduced from this assumption with regard to the sums of the measures.

With regard, however, to the parallax of A deduced from the measures of difference of distances of (a) and (a') from A, viz. 0″·135, M. Peters considers this result to be entitled to great confidence, since every source of error, whether arising from optical imperfections in the instrument, the unequal heating of the air in the tube, the difference of the temperature within and the temperature without, or the uncertainty in the value of the temperature-correction, will be almost completely eliminated.

Thinking, therefore, that the measures from which this result has been obtained deserve every care which can possibly be bestowed upon them, he has subjected them to a fresh reduction, according to a method formerly applied by him to the measures of 61 *Cygni*, so as to get rid altogether of the effect of any uncertainty in the temperature-coefficient; and in this manner he deduces for the most probable value of the relative parallax of A, or *Groombridge* 1830, 0″·141, with the probable error 0″·013.

Papers read before the Society from February 1853 *to February* 1854.

1853.

Mar. 11. Extracts from a Letter. M. Otto Struve.
On a Polishing Machine for Specula. Mr. De La Rue.
On the Measure of *Saturn*. Rev. W. R. Dawes.
Ditto ditto Mr. Lassell.
On the Uranography of *Saturn*. Dr. Lardner.
On the Visibility of *Jupiter's* Satellites, &c. to the naked Eye. Mr. Stoddard.
Letter from Valletta. Mr. Lassell.
Results of Catton's Observations. The Astronomer Royal.
Sur la Détermination de l'Orbite d'un Satellite. M. Sawitsch.
Note on the Form of the Planet *Saturn*. Rev. R. Main.

April 8. Observations of *Saturn*, &c. at Valletta. Mr. Lassell.
Discovery of a Comet. Prof. Secchi.
Measures of Double Stars. Mr. Lassell.
Measures of Diameter of *Saturn*. Mr. De La Rue.
On the Correction of the Apparent Ellipticity of a Planet. Mr. Grant.
On the Classification of Comets, &c. Dr. Lardner.
On a Mounting for a large Reflecting Telescope. Mr. W. Simms, Jun.
On the Light of the Star γ *Corvi*. Rev. T. Webb.
On the Theory of Clock Escapements. Mr. Bloxam.

May 13. Discovery of *Phocæa*. M. Valz.
Account of Tycho's Observatory. Mr. Wackerbarth.
On Solar Spots and Comet of March 1853. Prof. Secchi.
On a small Star near α *Canis Majoris*. Mr. Fletcher.
On the Red Prominences seen in Solar Eclipses. Rev. W. R. Dawes.
Discovery of a new Planet. M. De Gasparis.
Micrometrical Measurements of γ *Virginis*. Mr. Miller.
Copy of a Letter to the Astronomer Royal. M. Villarceau.
On the Orbit of First Comet of 1850. Mr. Carrington.
On *Saturn's* 8th Satellite. Mr. De La Rue.
On the Mechanical Imitation of Precession. Rev. Prof. Baden Powell.

June 10. Micrometrical Measurement of Double Stars. Mr. Miller.
On the Stability of the Planetary System. Dr. Lardner.
Remarks on *Saturn*, as seen at Madras. Capt. Jacob.
Micrometrical Measurement of Double Stars. Mr. Fletcher.
Parallax of α *Herculis*. Capt. Jacob.
Discovery of a new Planet by M. Luther. Prof. Argelander.

Determination of the Latitude of Liverpool Observatory. Mr. Hartnup.
Solution of Problem of Planetary Perturbation. Prof. Encke.
Occultation observed at Ashurst. Mr. Snow.
On the Photometry of the Stars. Rev. W. R. Dawes.
On a mode of Mounting a Telescope Equatoreally. Mr. Hopkins.
Observations of the Zodiacal Light. Mr. Burr.
Note on Paper, No. 372. Prof. Powell.
Observations of a Comet. Mr. Vine.
Ditto ditto Lieut. Goodenough.
Ditto ditto Mr. Maclear.
On the Comet of May 1853. Capt. Shadwell.
On the Zodiacal Light. Mr. Lowe.
Daylight Observations of Comet III. 1853. Mr. Hartnup.
Observations of Schweizer's Comet. Mr. Maclear.
On Rating Chronometers by Lunars. Capt. Toynbee.
Observations of the Dark Ring of *Saturn*. Mr. Burr.
On the Longitude of Cambridge. Mr. Airy.
Notes on Capt. Jacob's Observations. Prof. C. P. Smyth.
Correction of the Moon's Parallax. Mr. Adams.
On the Variable Star γ *Corvi*. Rev. T. Webb.

Nov. 11. Observations of Comet II. 1853. Mr. Constable.
Ditto on Comet III. 1853. Mr. Forster.
On the Longitude by Equal Altitudes of the Moon. Mr. Hedgcock.
Observations of *Saturn*. Rev. W. R. Dawes.
Ditto Comet II. 1853. Lieut. Parish.
Ditto ditto Capt. King.
Discovery of a new Planet. Mr. Hind.
On the Observatory at Red Hill. Mr. Carrington.
Extract of a Letter respecting the Lunar Tables from Prof. Hansen.

Dec. 9. Note on Comet III. 1853. M. Rümker.
Observations on Comet III. 1853. Mr. Bradshaw.
Description of his Star Maps. Mr. Carrington.

1854.

Jan. 13. Note on Cometary Orbits. Mr. Cooper.
Notes on the Zodiacal Light. Rev. T. Webb.
Remarks on the Aurora Borealis. M. Crestadoro.
Occultations observed at Ashurst. Mr. Snow.
Observations of Comet V. 1853, and of *Euterpe*. M. Rümker.
Catalogue of R.A. of 1009 Stars. Lord Wrottesley.
Observations of Nebulæ, &c. made at Valletta. Mr. Lassell.
Cosmical Speculations. Mr. Nasymth.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
Royal Society of London.
Royal Society of Edinburgh.
Royal Geographical Society.
Royal Asiatic Society.
Royal Institution.
Royal Irish Academy.
Geological Society.
Linnean Society.
Zoological Society.
Society of Arts.
The Photographic Society.
British Association.
Art-Union of London.
Institute of Actuaries.
The Chronological Institute.
Corporation of Glasgow.
Hon. East India Company.
The Registrar-General.
The Superintendent of the Nautical Almanac.
The Radcliffe Trustees.
Hon. Board of Ordnance.
L'Académie National des Sciences de l'Institut de France.
Dépôt Général de la Marine.
Bureau des Longitudes.
L'Académie des Sciences de Dijon.
Royal Academy of Munich.
Royal Academy of Berlin.
Royal Academy at Brussels.
Royal Academy at Göttingen.
Royal Academy of Sciences, Madrid.
Imperial Academy of Sciences at Vienna.
The Academy of Sciences at Dantzig.
The Academy of Sciences at Naples.
The Collegio Romano.
The American Government.
The American Coast Survey.
The Observatory at George Town College, U.S.
The Observatory at Munich.
The Observatory at Königsberg.
The American Philosophical Society.
The Smithsonian Institution.
The Franklin Institute.
The Calcutta Public Library.
The Editor of the Athenæum Journal.
The Editor of the Literary Gazette.

R. Abbot, Esq.
J. C. Adams, Esq.
G. B. Airy, Esq.
Prof. Argelander.
C. Babbage, Esq.
Adml. Sir F. Beaufort.
M. Biot.
Sig. Boncompagni.
M. Busch.
R. C. Carrington, Esq.
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E. Cheshire, Esq.
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J. F. Miller, Esq.
Dr. Morton.
H. Perigal, Jun. Esq.
Dr. Petersen.
Prof. Quetelet.
Prof. Rhiuonapoli.
Rev. Dr. Robinson.
Dr. Rutherford.
M. J. F. S. Schmidt.
Prof. A. Secchi.
Rev. B. Sestini.
Rev. R. Sheepshanks.
Prof. C. P. Smyth.
Prof. W. Von Struve.
Thos. Tate, Esq.
R. Taylor, Esq.
Dr. A. Vogel.
T. T. Wilkinson, Esq.
Chas. M. Willich, Esq.
Mr. J. Williams.
Lord Wrottesley.
Dr. I. Zech.

Address delivered by the President, G. B. Airy, Esq. F.R.S., Astronomer Royal, on presenting the Gold Medal of the Society to Mr. Charles Rümker.

Among the various duties, Gentlemen, which devolve on your President, there are none more agreeable than those which refer to the award of the Annual Medal. The discussions in the Council relating to this subject, if it were possible or proper that the public should become acquainted with them, would, I am confident, be matter of pride and satisfaction to the Society. It would be found that the literature, the mathematics, the instrumental constructions, and the observing labours of astronomy, have all engaged the careful attention of the Society's representatives. While the splendid discoveries which have excited the astonishment of the world have received their appropriate notice, the labours of technical character, little known to the public, but indispensable for the establishment of every interesting point in astronomy, have always successfully claimed the sympathy of the Council. Amid this apparent conflict of demands on the Council's attention, no dispute has arisen. Scarcely ever—I believe never—has it occurred

that in looking back to the award of our medals, we could say that any important subject had been omitted which ought to have been taken into consideration, or that a better choice could have been made than that which we have made.

From the list of persons that have been brought before the Council on the present occasion, Mr. Charles Rümker of Hamburgh has been selected as entitled to the Society's medal, generally for his long-continued labours in Observing Astronomy, specially for the Catalogue of 12,000 Stars, of which the last part was published nearly two years ago.

It is not necessary for me to describe at length the miscellaneous astronomical observations which we owe to Mr. Rümker, but a general notice of them may be thought necessary. Many years since Mr. Rümker first became extensively known by his connexion with the then infant Paramatta Observatory. Some misapprehension on one side or the other as to the precise terms of engagement brought the connexion to a close. I am totally unable to state with accuracy what was the point under discussion, or what were the merits of the two views of it; but I am certainly able to say that no greater misfortune has happened to southern astronomy than the conclusion of that engagement. Since that time Mr. Rümker has been known principally as Superintendant of the Nautical School of Hamburgh, and as Astronomer of the Hamburgh Observatory. It is more particularly in the latter capacity that his labours have attracted the attention of this Society. For a very long time Mr. Rümker has been known as furnishing extrameridional observations of comets and newly discovered planets, possessing the highest degree of accuracy, and extending to times when the objects which *he* could successfully observe were lost to other astronomers furnished apparently with much more powerful means. I have myself visited the observatory and inspected the instruments which have been devoted to these observations, and I have inquired, How is it that with instruments so insignificant, you have been able to see so much more than others could see who are so much better equipped? The answer was very simple. Energy, care, patience,—in these, I believe, is contained the whole secret. Mr. Rümker, perhaps, possesses in perfection the sensibility of eye and the acuteness of ear which are required for the most delicate observations; but these powers, which might seem at first to be original gifts of nature, have, I do not doubt, acquired very much of their activity from their careful and energetic use.

The observations to which I have referred possess that character which makes them known in some measure even to the unscientific part of the public. Those who are least initiated in the secrets of our noble science can appreciate, and with no failing approximation to their true value, the skill and the knowledge which instruct an astronomer, from observations of a new planet at the beginning of a summer, to say precisely where it will be at the end; or which enable him, after intently gazing upon an obscure speck which no

unarmed eye can see, to predict that ere long there will blaze in a definite portion of the sky one of those portentous objects which have made the hearts of whole nations to flutter. But the practical astronomer knows that, for the successful attaining to these results, much more is necessary which the public cannot appreciate. Above all, there must be extensive and accurate catalogues of stars. I might use a somewhat inaccurate architectural comparison, and say that these are analogous to the foundation of a building. I may adopt a much more exact engineering illustration, and say that they are closely similar to the bench-marks which the surveyor must lay down for his great hydraulic constructions. The traces of these commonly disappear from the eye. In looking at the lofty cathedral or the gorgeous palace, we seldom reflect on the care which has been required to establish the foundation; in viewing the aqueduct which supplies our cities with water, or the canal which bears its commerce, we scarcely think how great pains have been required, and have been given, for the determination of the heights of seemingly unimportant points, which perhaps the public in general never see. But, let the building sink in irreparable ruin, or let the levels of the canal be so faulty that its current is stopped, or overflows, or is turned in the wrong direction, and all the world can then understand how important was the omission of those preliminary preparations, on which, if successful, they would not have bestowed a thought.

The accurate determination of the position of numerous stars has always, in the flourishing times of astronomy, claimed the energetic attention of astronomers. In modern times it was Flamsteed who gave to this principle its most extensive developement. In estimating his labours, it must be borne in mind that he had himself to construct the appropriate instruments, that he had to determine fundamental points (commencing with those related to the sun's motion), as well as places merely referred to them, and that he calculated all the results and reduced them to a tabular form. The experience of subsequent years has shown that it is not easy, in an official observatory charged with peculiar responsibilities, to keep up this extent of cataloguing, observation, and reduction; but unceasing attention has been given at Greenwich to the determinations of the equinoctial zero and of the places of a limited number of stars. A nearly similar plan, sometimes with additions, has been followed at other principal observatories, as Königsberg, Cambridge, &c. and the catalogues thus formed, though restricted in the number of stars, have been the best bases for the extended catalogues of which I have now to speak.

It would be a long task to describe the different catalogues, reduced to different degrees of perfection, and exhibited in different forms, with which Astronomy is now enriched; and it would be invidious to attempt to apportion the different degrees of merit which I think ought to be ascribed to them. Still I cannot entirely avoid attempting this distinction, for without it I should

fail in conveying to you a proper idea of the estimation in which I hold the work to which your medal is awarded. In spite, then, of some defects arising from instrumental imperfections, I must characterise the Madras Catalogue of our late member, T. G. Taylor, as the greatest catalogue of modern times. In the number of observations and the number and distribution of the stars, and in the circumstance that the observations were made, reduced, combined, and printed, at the same place and under the same superintendance, it bears the palm from all others. But this was the fruit of an endowed observatory, the work of an astronomer and competent assistants, whose strength was not exhausted by any other employment. After this come such works as Groombridge's Catalogue (admirably observed, but reduced by its author only in the first degree), and many others, among which I will not attempt to discriminate. The Berlin and other star-maps, on which the points have been laid down by estimation merely, may be considered as in the same grade. The catalogues of lowest degree are those which consist merely of observations of zones of stars published in the order in which they are made, as the *Histoire Céleste*, Bessel's *Zones*, &c., whose form has been found so imperfect that following astronomers have been compelled, for alleviation of their own labour, to bring them to the state of a finished catalogue. I should be sorry to be understood as setting a low value either on the accuracy or on the extent of these zone-observations, but I do desire to convey my impression, of the imperfection of their form and the small expenditure of labour which they exhibit. Perhaps no one who is not familiar with the processes of observation, daily reduction, annual reduction, and combination of different years' places of stars, can conceive how trifling is the work of mere observation compared with that of reducing and cataloguing the results.

If, now, we estimate the work of Mr. Rümker by the rules which I have in some measure indicated, we shall find that it occupies a very high place. The number of stars in the complete Catalogue (more than twelve thousand) exceeds that of Taylor's. In the number of observations of each star, it is exceeded by Taylor's, but in almost every instance the number may be considered as sufficient. The observations have all been made with a first-rate meridional instrument. The stars, principally, but not entirely, in the northern hemisphere, have been selected with the special objects of filling up the gaps in the heavens left by the *Histoire Céleste* and Bessel's Zones, and of supplying accurate places of the comparison-stars employed in numerous extrameridional observations. The observations have been completely reduced, combined for one epoch, and printed in the form of an arranged catalogue. Every star is furnished with the star-constants which are necessary for reducing its mean to its apparent place (or *vice versâ*), in Bessel's manner. For those stars which are known or suspected to have sensible proper motion, the apparent place resulting from every individual observation is given. The

the work, observation, reduction, combination, and superintendence of printing, appears to have been done by Mr. Rümker himself; the preparation of the star-constants only, with some other minor parts of the calculations, being due to his friends Messrs. Weyer and Funk.

In my estimate of the value of this Catalogue, it is no small merit that, though intended to supply the defects in the extent of other works and thus to make a symmetrical whole, it has not made a symmetry of imperfections it has furnished: as supplement to two very incomplete publications, one of the most finished pictures of a large portion of the heavens which Astronomy possesses. It is also important that, while on the one hand, it possesses all the compactness of an epitomised Catalogue, on the other hand it furnishes in every case, in which it appears probable that the speculator on proper motions will require them, all the necessary details of individual observations. The annexation to the star's places of the constants for daily reduction, is an improvement which up to this time had been confined (so far as I am aware) to British Catalogues.

Had this Catalogue proceeded from an Observatory of which the personal establishment was charged with no other labours, we should have considered it as a highly meritorious work. The understood restriction on the disposal of our Medal would probably have prevented us from bestowing on it that last mark of our respect, but we should in our public report have alluded to it as one of the most admirable astronomical undertakings of its time. What, then, shall we say to this work in the circumstances under which it has reached us? It has come, the voluntary enterprise of an individual, who could not, by any construction of his connexion with the Hamburgh Observatory, be supposed to owe to the world a hundredth part of the labour which it has cost. It is the fruit of observations made in the watches of night, and calculations made in the leisure hours of the day, by a person who would seem, to vulgar eyes, to be engrossed to the limits of human endurance by an onerous professional office. Well may we consider it as a remarkable instance of voluntary labour, undertaken under difficult circumstances, not for public display, but as an aid to science, and skilfully and steadily directed to that purpose alone. I am confident that the award of the Council is supported by the approval of every member of this Society, and I will now place the Medal in the hands of our Foreign Secretary with a request that he will transmit it to Mr. Rümker.

The President then delivering the Medal to Mr. Hind, addressed him in the following terms:—

Mr. Hind,—I request that, in the name of the President and Council of the Royal Astronomical Society, you will transmit this Medal to Mr. Charles Rümker, as awarded to him for his astronomical observations in general, and for his Catalogue of twelve thousand stars in particular. No one knows better than yourself

the importance of these works, and the labour of completing them, and no one can convey in more appropriate language the sense of the Society on Mr. Rümker's claims to our respect.

In expressing to Mr. Rümker our friendly wishes for his personal happiness, there is one topic which ought not, I think, to be entirely omitted. We may well suppose that after so many years of severe labour Mr. Rümker may now be disposed to rest from his astronomical exertions. But the name is still connected with the daily progress of astronomy; our own country has secured for the present the active services of one whose astronomical knowledge and scientific habits are due entirely to *his* training, and in whose success he cannot fail to feel the warmest interest. In presenting our official mark of respect to the father, present also our congratulations on the prospects of the son, and our hopes that the names of both may long be connected with the progress of the science which has united us all.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

G. B. Airy, Esq. M.A. F.R.S. Astronomer Royal.

Vice-Presidents:

Arthur Kett Barclay, Esq.
John Lee, Esq. LL.D. F.R.S.
Rev. Robert Main, M.A.
Admiral W. H. Smyth, K.S.F. D.C.L. F.R.S.

Treasurer:

George Bishop, Esq. F.R.S.

Secretaries:

Augustus De Morgan, Esq.
Captain R. H. Manners, R.N.

Foreign Secretary:

John Russell Hind, Esq.

Council:

J. C. Adams, Esq. M.A. F.R.S.
Richard C. Carrington, Esq.
Rev. Thos. Pelham Dale, M.A.
Warren De La Rue, Esq. F.R.S.
George Dollond, Esq.
Rev. George Fisher, M.A. F.R.S.
James Glaisher, Esq. F.R.S.
Robert Grant, Esq.
William Rutherford, Esq. LL.D.
Rev. Richard Sheepshanks, M.A.

ROYAL ASTRONOMICAL SOCIETY.

VOL. XIV. *March* 10, 1854. No. 5.

ARTHUR KETT BARCLAY, Esq., V.P., in the Chair.

J. S. Ancona, Southampton Street, Bloomsbury; and James Cockle, Esq., of the Middle Temple,

were balloted for and duly elected Fellows of the Society.

Vol. xxii. of the *Memoirs* has recently been published. It contains three plates of engravings, besides a series of wood-cut illustrations, accompanying one of the papers. The price of the volume is 7*s.* to Fellows and 14*s.* to the public. Vol. xiii. of the *Monthly Notices*, which contains shorter communications, abstracts of papers, lectures, &c., is *given* to purchasers of the 4to. volume. The two publications are supplementary to each other, and are to be considered as part of the same series. They contain scarcely anything in common, except the Annual Report; and between them include a complete account of the proceedings of the Society during the year.

Extract of a Letter from M. Hansen to the Astronomer Royal.

"In respect to the *Astronomische Nachrichten*, I can communicate to you that the Royal Danish Government has provisionally voted the grant for the completion of the volume now in the press, and that I shall provisionally continue the editing of the work. I, therefore, request you, as well as all other English astronomers, to have the goodness to send to me all communications for this journal."

Observations of Comet III., 1853.

The following observations of a comet (apparently Comet III, 1853) seen at Auckland, New Zealand, on board H.M.S. Calliope, were made by Mr. G. P. Heath, acting under directions of Capt. Sir J. E. Hare, Bart., F.R.S., and were transmitted to Capt. King, of Paramatta, N.S.W., who communicated them to the Society through the Rev. Geo. Fisher.

"The comet was first seen about 4 A.M. upon the 13th September, very distinctly. The morning of the 14th was cloudy. On the 15th, at $5^h\ 11^m$ A.M., between two squalls it was again seen. It was colourless. The length of the tail was 4° 45′, gradually widening from the head; the extreme width about ½°; produced, it would pass about 10° to the south-eastward of *Canopus*.

tions were fixed by distances from *α Crucis* and *Canopus*, and are as follows:—

1853.	M.T. h m	R.A. h m s	Decl. South. ° ′ ″	Long. East. ° ′
Sept. 14	17 11	10 24 49·3	20 7 49	4 45
16	16 51	17 54·6	22 24 49	2 0
20	16 22	8 22	25 56 15	
21	16 49	5 57·5	26 40 16	
24	16 33	10 1 14·5	28 32 12	3 0
27	16 29	9 57 14·5	30 7 24	
Oct. 10	15 41	9 45 8·4	34 50 47	2 0

"On the mornings of the 17th, 21st, and 22d, less of the tail was seen, from the haziness of the atmosphere and the brightness of the moon."

Observations made by Mr. Heath with a Sextant, to ascertain the Position of the Comet: corrected for Index Error.

		° ′ ″		° ′ ″
Sept. 14	Comet to Canopus	56 39 45	Comet to α Crucis	46 28 0
16	——	53 52 45	——	44 49 0
20	——	49 47 30	——	42 16 45
21	——	48 53 10	——	41 48 10
24	——	46 48 10	——	40 29 60
27	——	45 3 30	——	39 25 0
Oct. 10	——	39 55 50	——	36 22 0

Observations of a Comet seen in the Southern Hemisphere.

(*Extract of a Letter from Captain Drury, of H.M.S. Pandora, to Rear-Admiral Sir Francis Beaufort.*)

"The following observations were made in the Pandora, off Norfolk Island, in latitude 29° south, longitude 168° east, on Sept. 13, 1853, at $4^h\ 45^m$ A.M.; and in the Hauraki Gulf, New Zealand, on Sept. 25th, $3^h\ 45^m$ A.M.

"To the naked eye the nucleus first appeared four times the size of *Jupiter*, of a pale, but considerable brightness, as if the light of *Jupiter* were spread over four times its surface. The tail was about 5° in length, making an angle with the vertical circle of 15° south of the zenith. It was seen in the constellation of *Crater et Hydra*. Its brightness diminished daily, and it was last seen on Sept. 25th in the constellation of *Anthia Pneumatica*. Its right ascension on the 13th, at $4^h\ 45^m$ A.M., was (as measured from three stars) $10^h\ 32^m\ 32^s$, and its declination 17° 2′ 30″ south. From the same number again on the 25th its position was in right ascension $9^h\ 59^m\ 14^s$, and declination 27° 51′ 0″ south, by which it appears to have travelled through 2′ 48″ of arc per hour.

"The clouds prevented any further measurements, but I am enabled to fill up most of the intervening days by the observations of Mr. Heath, acting mate of the Calliope, observed on board at Auckland.

Date. 1853.		R.A.	Decl. S.	Length of Tail.	Ship.	Observer.
	h m	h m s	° ′ ″	° ′		
Sept. 12	16 45	10 32 32	17 2 30	5 0	Pandora	Capt. Drury
24	15 45	9 59 14	27 51 0	...	—	Mr. J. H. Kerr, Second Master.

"Government House, Flag Staff, Norfolk Island, mean latitude by forty circum. mer. alts. 29° 2′ 39″ south. Mean longitude by eleven chronometers, rated at Auckland before and on return, 168° 0′ 47″ east. Measured from Auckland, longitude Dépôt Point, 174° 50′ 40″ east." *

New Planet discovered at Mr. Bishop's Observatory by Mr. Marth, 1854, March $1^d\ 14^h\frac{1}{2}$.

1854.	G.M.T.	App. R.A.	App. N.P.D.
	h m s	h m s	° ′ ″
March 1 at	14 45 7	13 17 31·06	100 5 29·8
2	14 53 48	13 17 4·61	100 5 25·4

Shines as a star of 10.11 magnitude.

The planet was also discovered independently by Mr. Norman Pogson, Assistant at the Radcliffe Observatory, Oxford, from two observations made on the 2d of March. The second of these observations was only nine minutes later than the observation of the same day made by Mr. Marth. The same impression of the *Times* contained two independent communications, from Mr. Hind and Mr. Johnson, each containing the announcement of the discovery. The planet has received the name of *Amphitrite*.

Discovery of a New Planet. By M. Luther.

A new planet was discovered by M. Luther at the Observatory of Bilk, on the 1st of March, 1854. It has been called *Bellona*. The following elements of its orbit have been calculated by Professor Chevallier and Mr. George Rümker :—

Epoch, 1854, March 15·0.

M	47 34 34·3	
ϖ	113 4 19·1	Mean Equinox,
λ	146 27 12·8	1854·0
i	10 5 22·2	
φ	8 53 56·0	
Log a	0·449,298	
Log μ	2·867,060	

These elements are based on the following observations :—

BILK.

1854.	Bilk M.T.	R.A.	Decl. N.
	h m s	° ′ ″	° ′ ″
March 1	12 24 24	181 23 57·3	7 [illegible]

* Mr. Heath's observations are here omitted, as they are contained in the paper immediately preceding, communicated [illegible]

Mean of the following two observations:—

HAMBURG.

	Hamburg M.T.	R.A.	Decl. N.
	h m s	° ′ ″	° ′ ″
March 4	10 21 41	180 55 52·9	7 28 46·2

BONN.

		R.A.	Decl. N.
		° ′ ″	° ′ ″
March 4	Meridian	180 54 29·0	7 29 48·0

DURHAM.

	Green. M.T.	R.A.	Decl. N.
	h m s	° ′ ″	° ′ ″
March 11	13 7 33	179 40 24·6	8 35 10·7

Professor Chevallier has sent to the Society, for distribution, several printed copies of an ephemeris of the planet calculated from the foregoing elements, and extending from March 15 to April 16.

A NEW COMET.

A new comet has been discovered, which became generally visible to the naked eye on the 29th of March. Mr. Carrington writes word that it was first seen by him, and unexpectedly, at about $7^h\,38^m$ G.M.T., on the 29th, and makes the same remark that others have done respecting its general similarity to the one of August last. He obtained two pretty good comparisons with the star B.A.C. 348 before the comet set, and hastened to London on the morning of the 30th to circulate them, when he found that the comet had been already seen elsewhere. He too observed it on the 29th, 30th, 31st of March, and on the 1st and 2d of April; but the observations will not be fully reduced in time for the present *Notice*. From the following three positions, approximately made out for the purpose, he has computed the subjoined parabolic elements:—

1854.	h m s	R.A. h m s	N.P.D. ° ′ ″
March 29	8 0 13·7	1 6 22·48	70 12 13·0
30	8 7 22·7	22 0·40	22 12·0
31	7 32 3·4	1 36 56·33	70 44 8·6

T	1854, March $24^d\cdot13742$, Green. M.T.	
	° ′ ″	
π	211 43 27	M. Eq. 1854·0
Ω	313 44 20	
I	81 39 6	
Log q	$= 9\cdot443310$	Retrograde.

Notice of the Comet. By E. J. Lowe, Esq., at Highfield House Observatory, near Nottingham.

"I discovered a large and brilliant comet this evening (March 29th) at $7^h\,50^m$, which was situated a few degrees above the N.W. by W. horizon. Clouds in the neighbourhood prevented me marking its position as accurately as I could have wished, and the want of an equatoreally-mounted telescope renders the position a very rough estimate. It appears to be situated on the border of *Pisces*, not far from γ *Andromedæ*. The nucleus was equal to a star of the

2d magnitude, and the tail (which extended in the direction of *Polaris*) was about 4° in length.

Elements of the comet from three observations at Mr. Bishop's Observatory, on March 29, 30, and 31. By J. R. Hind, Esq.

Per. Pass. 1854, March 24·33842, G.M.T.

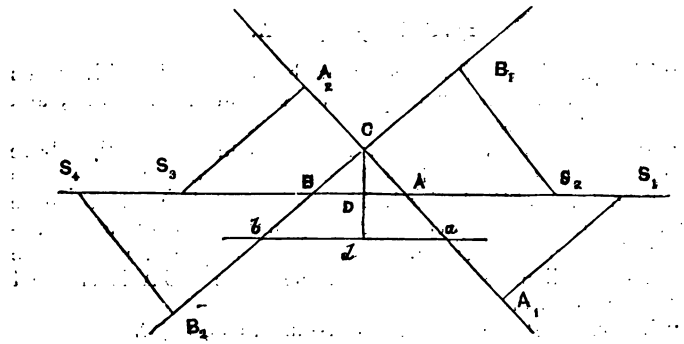
π	207 34 8	T.E., March 30
Ω	310 22 32	
i	79 27 14	
log q	9·445695	

Retrograde.

On a Method of Observing the Positions of Spots on the Sun; with an Example of its Application. By R. C. Carrington, Esq.

"The following method occurred to me in the month of October of last year, and has been made use of at Redhill on every subsequent occasion on which the sun has been visible. The advantages for which I would recommend it are, that it requires no previous accurate adjustment of a wire frame; that direct vision of the sun is unnecessary; that the contacts by which the position of the centre is determined are thrown to the beginning and end of the observation, so that an uninterrupted space of about three minutes is given for observing the contacts of the spots; and that the whole observation takes so little time that records may be procured on days of broken weather, when it would often be impossible otherwise to get an observation at all.

"In the focus of the equatoreal are placed two wires at right angles, turned *approximately only* into the position of 45° inclination to a parallel of declination, and the image of the sun's disk and the cross-wires is projected on a screen, placed at such a distance that the projection of the disk may measure from 12 to 14 inches. The most effective screen I have yet tried consists of a plate of ground glass, covered with whitewash, tinted a pale straw colour, with dark cloth or dead-black cartridge paper placed behind. The sun is, of course, seen to run along the screen from



right to left, the true north limb being the upper limb on the screen

" In the figure, the sun's centre is supposed to run along the line $S_1 S_4$, passing south of the intersection point C; and $a\,b$ to be in like manner a portion of the path of a spot. When the centre has arrived at a point S_1, such that the length of the perpendicular from S_1 on either of the wires, as $S_1 A_1$, is equal to the radius of the disk, a contact of the disk with the wire will take place, the instant of which is to be observed. There will be four such contacts in all, the times of which are conveniently denoted by the letters A_1, B_1, A_2, B_2.

" The line C D d is supposed to be dropped perpendicular on the paths of the centre and of the spots.

" In the first instance, for simplicity, neglecting the effects of refraction, of hourly motion, and of error of verticality of the wires, we shall have by the geometry of the case,

$$\frac{BC}{2 \cdot AB} = \frac{S_1 A_1}{S_1 S_3} = \frac{r}{15 \cdot \sin \delta\,(A_2 - A_1)}$$

$$\frac{AC}{2 \cdot AB} = \frac{S_2 B_1}{S_2 S_4} = \frac{r}{.15 \sin \delta (B_2 - B_1)}$$

whence $$\frac{BC}{AC} \text{ or } \tan BAC = \frac{B_2 - B_1}{A_2 - A_1}.$$

" The centre passes A at $\frac{1}{2}(A_1 + A_2)$, and B at $\frac{1}{2}(B_1 + B_2)$; and, therefore, from A to B in the time $\frac{1}{2}(B_1 + B_2) - \frac{1}{2}(A_1 + A_2)$.

$$AD = AB \cdot \cos A \cdot \cos A$$
$$CD = AB \cdot \cos A \cdot \sin A$$

" Let T stand for $\frac{1}{2}(A_1 + A_2) + AD$, and D for C D; t and d being similar quantities for a spot. Then the difference of R.A. in arc, and of N.P.D. for a spot, as compared with the sun's centre, will be

$$15 \cdot \sin \delta \cdot (t - T) \text{ and } 15 \sin \delta\,(d - D),$$

and the diameter of the sun will be

$$15 \cdot \sin \delta\,(B_2 - B_1) \cos A.$$

" The quantities A B and $a\,b$ must be considered positive when the contact at the A a wire precedes that at the B b wire, and negative in the opposite case.

" In very many instances a rough calculation on the above principle will be thought sufficient; but where two or three passages have been favourably obtained, the accuracy of the method will be found to warrant a more precise reduction in the manner following:—

" 1. The error of verticality of the wires may be found by the comparison of diameter results, when the angle B C A is changed for its supplemental angle.

" Let one angle be marked and assumed $= 90° + \iota$.

" Supposing the wires to be adjusted not much out of the

described position, there will be no difficulty in seeing that the expression for the diameter will be

$$2r = 15 \cdot \sin \delta \cdot (B_2 - B_1) \cos A \cdot (1 - \tfrac{1}{2} \sin \theta).$$

" If the unmarked angle be now substituted, we shall have

$$2r = 15 \cdot \sin \delta \cdot (B'_2 - B'_1) \cos A' \cdot (1 + \tfrac{1}{2} \sin \theta);$$

from which θ may be found, if desired. The quantity actually required is the factor $(1 - \frac{1}{2} \sin \theta)$, which we will denote by the letter G, and by which all our results must be multiplied.

" 2. The effect of refraction may be taken into account with sufficient accuracy by assuming every point on the disk refracted upwards in a line parallel to the vertical circle through the centre, and that the difference of refraction of any two points in the same such line is proportional to their distance. In this case, the disk will be strictly elliptic; and, in place of the radius in the previous expressions, we must substitute the expression for the perpendicular from the centre of an ellipse on its tangent.

" 3. The effect of the hourly motion is disposed of by the consideration that, as the results are independent of the angle at which the sun's path cuts the wires, the motion in R.A. will affect the scale of the results only, and the motion in N.P.D. the angles only. We shall, therefore, include their effects by multiplying all our results by the well-known factor $\frac{3600}{3600 + I}$; and, by a slight transformation of co-ordinates for I', the motion in N.P.D.

" The accurate formulæ will, accordingly, stand thus:—

$$F = 15 \cdot \sin \delta \cdot \frac{3600}{3600 + I} \qquad G = (1 - \tfrac{1}{2} \sin \theta)$$

$$\tan \alpha = \frac{B_2 - B_1}{A_2 - A_1} \qquad \tan \alpha' = \tan \alpha \cdot \sqrt{\frac{1 - e^2 \cdot \cos^2 (\alpha + s)}{1 - e^2 \cdot \sin^2 (\alpha + s)}}$$

in which s is the angle contained by the great circles drawn from the sun's centre to the pole and the zenith, and is to be considered + for east hour-angles, and — for west; and in which e is the eccentricity of the disk.

$$e^2 = l (2 - l) \text{ where } l = k \cdot \sec^2 z,$$

k being Bessel's factor for differential observations. As before,

$$AB = \tfrac{1}{2} (B_1 + B_2) - \tfrac{1}{2} (A_1 + A_2)$$
$$AD = AB \cdot \cos \alpha' \cdot \cos \alpha'$$
$$CD = AB \cdot \cos \alpha' \cdot \sin \alpha'$$

$$\Delta \alpha = (t - T) \cdot F \cdot G \qquad \Delta \delta = (d - D) \cdot F \cdot G.$$

" To these we must apply the corrections

$$+ \Delta \delta \cdot \frac{2 I'}{10^5} \text{ in R.A., and } - \Delta \alpha \cdot \frac{2 I'}{10^5} \text{ in N.P.D.}$$

for the hourly motion I', which is to be considered +, when the sun's N.P.D. is increasing.

" Lastly, the corrections,

$$k \sec^2 z \,.\, \sin s \,.\, (\Delta\alpha \,.\, \sin s + \Delta\delta \,.\, \cos s) \quad \text{in R.A.}$$

and $$k \sin^2 z \,.\, \cos s \,.\, (\Delta\alpha \,.\, \sin s + \Delta\delta \,.\, \cos s) \quad \text{in N.P.D.}$$

for difference of refraction.

" The sun's diameter will be

$$(B_2 - B_1) \,.\, F \,.\, G \,.\, \cos\alpha' \,.\, \frac{1+k}{\sqrt{1-e^2\sin^2(\alpha+s)}}$$

" As an example of the application of the formulæ, I have selected the observations of Nov. 25, 1853, the spots on that day being only two in number, and so situated as to illustrate the signs of the different quantities.

" The spots are denoted P and Q, and the times of passing the A a wire by P_a and Q_a, and of the B b wire by P_b and Q_b.

	Bar. 29in·85	Therm. 40°·3	Means.
	h m s	h m s	h m s
B_1	17 25 43·0	17 29 30·9	17 34 14·1	17 29 49·33
A_1	25 50·0	29 37·0	34 20·0	29 55·67
P_a	27 27·8	31 15·1	35 58·0	31 33·63
P_b	27 44·1	31 32·0	36 14·9	31 50·33
Q_b	27 45·4	31 33·4	36 16·2	31 51·67
Q_a	28 3·1	31 50·8	36 34·0	32 9·30
B_2	28 59·3	32 47·0	37 30·0	33 5·43
A_2	17 29 7·0	17 32 54·1	17 37 37·3	17 33 12·80

" The steps of the reduction will be as follow:—

	h m s
$\frac{1}{2}(B_1+B_2)$ =	17 31 27·38
$\frac{1}{2}(A_1+A_2)$ =	17 31 34·24
Mean = sun's transit =	17 31 30·8
Clock slow	14·4
	17 31 45·2
Add for longitude	41·3
	17 32 26·5

whence Greenwich mean time of observation, 1853, Nov. 25, $1^h\,14^m\,52^s{\cdot}5$. By the *Nautical Almanac*,

$\alpha = 16^h\,4^m\,50^s$ $\quad$ I = +10″·65 $\quad$ $\delta = 110°\,49'{\cdot}3$ $\quad$ I' = +28″·7

whence, $z = 74°\,33'$ $\quad$ $h = 1^h\,27^m$ west $\quad$ $s = -23°\,56'$

" With zenith distance, barometer, and thermometer as arguments,—

Log k = 6·4349

In our case Log G = 0·00010

whence, Log F . G = 1·14557

B_2-B_1 = 196″·10

A_2-A_1 = 197 ·13 whence log tan α = 9·99772 α = 44° 51′

ε = −13 56

$\alpha+\varepsilon$ = 30 55

Log e^2 = 7·8839

whence, Log $(1-e^2\cos^2(\alpha+\varepsilon))$ = 9·99755

Log $(1-e^2\sin^2(\alpha+\varepsilon))$ = 9·99912

Half the diff. =9·99922, which added to log tan α gives log tan α' =9·99694; whence α' =44° 47′·9.

A B = −6″·860			Semidiameter.
Log A B = −0·83632			$\frac{1}{2}(B_2-B_1)$ = 98·050
Log cos α' = 9·85101			Log = 1·99145
−0·68733	−0·68733		L cos α' = 9·85101
L cos α' 9·85101	L sin 9·84795		Log F . G = 1·14557
−0·53834	−0·53528		Log $(1+k)$ = 0·00012
−3·454			1·98815
17 31 34·235			Log √..... = 9·99956
T = 17 31 30·781	D = −3·430		2·98859
			½ diam. 974″·1
$\frac{2\,I'}{10^5}$ = +·0006			By *Naut. Alm.* 975 ·0

Spot P.		Spot Q.	
$a\,b$ = +16″·700		$a\,b$ = −17″·630	
Log $a\,b$ = +1·22272		Log $a\,b$ = −1·24625	
L cos α' 9·85101		L cos α' = 9·85101	
+1·07373	+1·07373	−1·09726	−1·09726
L cos α' 9·85101	L sin 9·84795	L cos α' 9·85101	L sin 9·84795
+0·92474	+0·92168	−0·94827	−0·94521
+8·409		−8·877	
P_a = 17 31 33·630		Q_a = 17 32 9·300	
t = 31 42·039	d = + 8·350	t = 32 0·423	d = − 8·815
T = 31 30·781	D = − 3·430	T = 31 30·781	D = − 3·430
t−T = +11·258	d−D = +11·780	t−T = +29·642	d−D = − 5·385
Log F . G = 1·14557	and ε = −13° 56′	Log F . G = 1·14557	and ε = −13° 56′
$\therefore \Delta\alpha\,.\sin\varepsilon+\Delta\delta\,.\cos\varepsilon$ = +122″ whence		$\therefore \Delta\alpha\,.\sin\varepsilon+\Delta\delta\,.\cos\varepsilon$ = −173″ whence	
$\Delta\alpha$ = +157″·4	$\Delta\delta$ = +164″·7	$\Delta\alpha$ = +414″·5	$\Delta\delta$ = −75″·3
Corr. for I′ + 0·1	− 0·1	Corr. for I′ − 0·0	− 0·2
Diff. refr. − 0·1	+ 0·5	Refr. + 0·2	− 0·6
+157·4	+165·1	+414·7	−76·1

"The corrections applied to $\Delta\alpha$ and $\Delta\delta$ may appear small, but in cases where the spots lie further from the centre they are more necessary. It will further illustrate the method to exhibit the

results of each passage separately, reduced without correction for refraction on the quantity I'.

	½ Diam.	Spot P.		Spot Q.	
	′	″	″	″	″
1st set	972·1	$\Delta\alpha$ +156·1	$\Delta\delta$ +165·3	$\Delta\alpha$ +411·2	$\Delta\delta$ −72·3
2d —	971·9	158·8	164·3	417·6	75·5
3d —	971·9	156·4	164·3	415·4	78·3

"I am not aware that the above sets are better or worse than usual, or that there exists any reason why they may not be taken as a fair sample.

"I think the time required for the whole observation will speak for itself, and I hope that the degree of accuracy attained will induce some other observer to think the method worthy a trial.

"As a last word, I would just point out that there is one objection to the use of cross bars instead of cross wires, which some might otherwise prefer. It not unfrequently happens that a group of spots of some extent lies nearly on the parallel of the sun's centre. In such a case the large space taken up by the crossing of the bars will prevent all the contacts being taken at the same passage, unless the centre be thrown to an objectionable distance from the point of intersection."

On some peculiar Features in the Structure of Lunar Volcanic Craters. By James Nasmyth, Esq.

In pursuing my investigations of the features of the lunar surface, aided by my very convenient 20-inch aperture reflecting telescope, I have been much interested in observing the details of what have hitherto been termed "terraces," namely, certain concentric ridges around the interior walls of the lunar volcanic craters; and as their peculiar features appear to me to throw some additional light on the nature of the action which had caused the formation of those remarkable "ring mountains," and which I take to be in reality the craters of extinct volcanoes, a few remarks on the subject may, perhaps, prove acceptable.

On a careful scrutiny of these concentric ridges above alluded to, I find that they are by no means continuous or regular in respect to height or position, but, on the contrary, are indented and broken, and that in many cases they form but a small segment of the interior wall of the crater; while, in other cases, portions of these "terraces" have slipped down, and that the flat bottom or plateau of the crater at the spot is covered with the *débris* of portions of the interior wall,—in short, that these features are the result of vast "*landslips*," which, when occurring to any great segment of the inner wall of the crater, yield the "terrace-like" appearance, while in a more limited extent they leave a notch or recess in the bank or cliff, and the *débris* seen at the base are the fragments of the landslip, many portions of which may be traced scattered about and projected inwards towards the centre of the

plateau. When these features are carefully examined with this "landslip" idea present in the mind, the evidences of such being the true cause and nature of their appearance become irresistible; inasmuch as that any one who has seen "landslips," and the fallen-in interior walls of volcanic craters, will be struck with the similarity of the aspect of the features in question.

With respect to the cause and the nature of the action which appears to me to have occasioned the features in question, I consider that the most probable explanation is, that during the most active state of those volcanic craters the matter ejected from the volcano accumulated, and was piled up to such a height on the summit of the great circular wall or crater, as by its crushing-down action, aided by the undermining and softening action of the intensely hot lake of molten lava (then occupying the bottom of the crater), the vast superincumbent mass forming the wall of the crater had, from time to time, and to a greater or less segmental extent, crushed and slid down, and so settled in a more permanent position: when occurring to a great segment, resulting in the "terrace-like" appearance; and when in a more limited extent yielding all the well-known features of a "*landslip.*"

It occurs to me, that if any one (having suitable telescopic means) will carefully examine the details of these features, they will not fail to regard these remarks as worthy of consideration, as yielding what appears to me a rational explanation of their nature and origin.

I may state that I consider the era of the active condition of the lunar volcanoes to have gone by ages ago.

Extract of a Letter from M. Otto Struve to the Astronomer Royal, dated January 17, 1854.*

"In the last months I have been so much engaged with other things that I could not find leisure to calculate my observations on the parallax of 61 *Cygni*. But Prof. Woldstedt being here on a visit in the month of September, has made a preliminary calculation, which gives

By the distances, the parallax	$= +0''.542 \pm 0''.033$
By the positions,	$= +0''.504 \pm 0''.045$

or, in the mean, a parallax greater by nearly one-third than Bessel's value. The proper motion deduced from my observations agrees within one or two hundredths of a second with that deduced from meridian observations. It was a very curious fact that, when first Woldstedt, by application of proper motion, had reduced the measures to one epoch, I was able to point out, by a single view, the epochs of the different maxima and minima, so exactly that the afterward calculated real epochs did in no case differ from my estimate by more than ten days. I do not regard Prof. Woldstedt's

* For a further extract from this communication, see "

calculation as definitive, but the ultimate corrections to be applied to the values he has found, I suppose, will not exceed 0″·02. How to explain the difference between Bessel's determination and mine is more than I can say at present. Perhaps it has originated merely from an accumulation of accidental error in opposite directions for the two series; but, more probably, it might be attributed in part to the action of the same uncommon causes, which evidently have disturbed in much higher degrees Wichmann's observations, and about which Doellen's researches contain very curious indications.

"In speaking here of constant or systematical errors, to which heliometrical measures are subject, it would not be right to avoid mentioning that likewise my own measures, made with the wire-micrometer, are not free from similar errors. On the contrary, a long series of experiments, made on artificial double stars, indicate very important corrections to be applied to all my measures of *close* double stars. From a great number of observations I have deduced the following empirical formula for the correction of positions, measured by me on double stars, 0″·8 to 6″ of distance :—

$$\text{Corr.} = +\frac{4^{\circ}\cdot 23}{\alpha} + 3^{\circ}\cdot 24 \sin(2\varphi - 29^{\circ}\,11') + 1^{\circ}\cdot 51 \sin(4\varphi - 31^{\circ}\,29')$$

"In this formula α is the distance expressed in seconds and φ the angle between the true direction of the double star and the vertical circle passing through the star at the time of observation.

"At about 6″ of distance the method of observation changes. It must be attributed to this circumstance, that for greater distances the coefficients of the empirical formulæ diminish rapidly, so that, at a distance of 12″, the systematical errors disappear altogether, or are incomparably smaller than the little accidental errors of observation. Hence it follows that my determination of parallaxes is quite free from these systematical errors.

"The constant errors of the measured distances are, generally speaking, less important, as it might be seen from the following list :—

True Dist.	Corr. of Meas. Dist.	True Dist.	Corr. of Meas. Dist.
″	″	″	″
0·99	+0·029	7·88	+0·094
1·94	0·079	11·60	+0·032
3·88	+0·136		

"I think the origin of all these errors in the measured positions is quite physiological, and it is highly probable that all practical observers will be subject to similar systematic errors. It requires no demonstration, that similar corrections will change *very considerably* all hitherto deduced elements of the orbits of double stars, especially as these are commonly deduced from the observed positions. Therefore, in future times, no other observations ought to be employed for that purpose, but those for which the personal corrections are well ascertained by direct experiments or by comparison with corrected observations.

"One other consequence which might be deduced from the existence of such errors is, that we must be very sceptical with regard to parallaxes deduced in the way in which it had been done by Capt. Jacob for *α Herculis*; most probably the parallax he has found in this case is nothing more than a strong indication that the positions observed by him are subject to similar systematic errors as mine: for in itself it is highly improbable that *α Herculis* and its companion should have different parallaxes. My researches on the constant errors of my observations are not yet closed, for there are yet many questions undecided. I shall continue them most probably throughout the whole ensuing summer, and only when they are finished I shall proceed to the publication of my fifteen years' observations of double stars."

On Two New Theorems in Lunar Astronomy. By Professor Challis.

(*Extract of a Letter from Professor Challis to the Editor.*)

"I have succeeded in demonstrating, on the principle of gravitation, two new theorems relating to the moon's orbit.

"Theorem I. All small quantities of the second order being taken into account, the relation between the radius vector and the time in the moon's orbit is the same as that in an orbit described by a body acted upon by a force tending to a fixed centre.

"If μ be the sum of the attractions of the earth and moon, and m' the attraction of the sun at the unit of distance, and if r be the moon's radius vector, and a' the sun's mean distance from the earth, the central force is $\frac{\mu}{r^2} - \frac{m' r}{2 a'^3}$. The theorem is true inclusive of the moon's motion in latitude.

"The integration by which the relation between the radius vector and the true longitude on the orbit is obtained, gives directly the mean motion of the apse to the first approximation.

The same integration involves the following consequence:—

"Theorem II. The excentricity of the moon's orbit is a function of the ratio of the moon's periodic time to the earth's periodic time, and, to the first approximation, is equal to that ratio divided by the square root of 2.

"This theorem applies to the *constant* part of the excentricity, and depends entirely on the circumstance that the apse has a mean motion.

"The proofs of these theorems are contained in a communication to the Cambridge Philosophical Society.

"*Cambridge Observatory, March* 9, 1854."

An Account of the Hartwell Rectory Observatory.

By the Rev. C. Lowndes.*

"It might be thought that an Observatory on the Rectory grounds at Hartwell would be quite unnecessary, as Dr. Lee has

* A model of this observatory has been sent to the apartments of the Society, which Dr. Lee explained at the last Meeting.

such an excellent one so near, to which, out of pure love of and to advance the science, he has given me, and indeed all who take an interest in astronomy, free access. But, as in the spring of 1849, I had commenced taking meteorological observations for the British Meteorological Society, of which I have been a member from its formation, I was very anxious to have an observatory of my own, and decided upon building a very *small* one. My friend and neighbour, the Rev. J. B. Reade, mentioned my intention to Dr. Lee, when he very generously and kindly said, if I would build a larger one, he would give me all the materials. Such an offer I was only too glad to accept, and, accordingly, I set about the work of building a transit-room, intending, when circumstances would allow, to add an equatoreal-room. A committee was formed at Dr. Lee's suggestion, consisting of himself, the Rev. J. B. Reade, and T. Dell, Esq., to fix upon a site, and to superintend the work.

"The site is the best, under the circumstances, which could be chosen, and commands the passage of *Fomalhaut* to the south. As no distant object can be seen, and there are no meridian marks, I am compelled to adjust the collimation by the passage of *Polaris*, and thus obtain the error.

"*The transit-room* is substantially built of *brick*, upon a *solid bed of concrete*, and is 10 feet square in the clear, and 8 feet 9 inches high, the top being covered with lead and the two shutters with copper. The floor is supported by joists, forming square frames round the transit and clock-piers, so that no tremor can be communicated by the planks; and the clock and two transit-piers are also placed upon *solid beds of concrete*, independent of the walls of the building. The two piers supporting the transit instrument are of *Caen stone*, 4 feet 11 inches above the floor.

"The transit instrument was made by Mr. T. Slater, optician, No. 4 Somers Place West, New North Road, London, and is supported upon brass Y's by two cones, which form an axis of 2 feet 5 inches in length, the pivots which rest in the Y's being made of bell-metal $1\frac{3}{4}$ inches in diameter. Two adjustments are attached to the Y's, the one on the west pier for making the axis horizontal, the other on the east pier for adjusting the telescope in azimuth. The telescope is 5 feet 7 inches in length, having an object-glass, whose aperture is $4\frac{3}{4}$ inches in diameter. There are five fine vertical wires and six thicker ones, for taking faint objects, placed in the principal focus, the intervals by *Polaris* of the five fine wires being $10^m 44^s$. The *setting circles* are fixed at the eye-end of the telescope; they are 6 inches in diameter, and are divided on silver. There is also a dew-cap fixed at the object-end, made of zinc, densely blackened within.

"The *level* used for adjusting the horizontal position of the axis of the transit instrument was also made by Mr. T. Slater; the value of each division is one second in space.

"*The clock*, which is placed in the south-west corner of the building, was made expressly for me by Mr. Dent; and a very excellent one it has proved. It has a compensating pendulum, and

a particularly good beat, and keeps an equitable losing rate; the variation on its rate being (invariably) less than one-hundredth of a second. It has a tendency to gain this quantity in the cold weather, and to lose it in the warm weather."

The foregoing description was accompanied by an account of the meteorological department of the Observatory and a detailed statement of the expenses connected with the undertaking. The following is the recapitulation on the latter head:—

	£	s.	d.
Cost of the building	55	0	9
Cost of astronomical instruments	138	10	0
Cost of meteorological instruments	17	2	0
Total	210	12	9

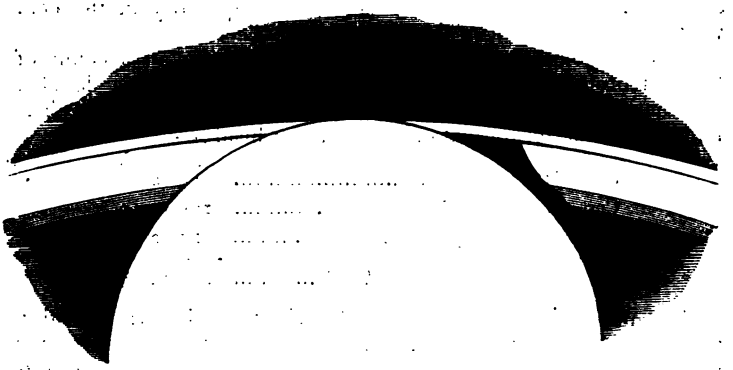
The paper also contained a few transits taken by Mr. Lowndes, at his Observatory; and a few similar observations made at the Observatory of Hartwell House by Dr. Lee's Assistant. The results for the several wires are very accordant in both cases.

Remarkable Appearance of the Shadow of Saturn projected on the Ring. By J. Hippisley, Esq.

"I observed *Saturn* near, and on the meridian, with a Newtonian equatoreal of $9\frac{1}{4}$-inch aperture, powers 360 and 462. The atmosphere afforded conditions of very unusual definition. I perceived distinctly a faint line or belt on the outer ring, at about three-fifths of its breadth from the inner edge. This line did not at all give the impression of a *division*; but as compared with the tint of the main separation of the rings, was, though of sensible breadth, of a much paler and fainter shade. Having on a previous occasion of similar, but equally rare, atmospheric tranquillity, had the advantage of viewing *Saturn* with Mr. Lassell's 20-foot telescope, and recalling some other circumstances of even superior definition with which the planet, as to its other features, was then seen, I feel assured that the outer ring was not at that time marked by any similar belt or line; and that those markings are therefore of a variable character.

"But the most remarkable feature was the shape and position of the shadow of the ball. This was conspicuously curvilinear, with the convexity towards the ball; and of such curvature that the curve produced ought to have passed over a considerable portion of the outer ring, if that ring had been in the same plane with the inner. It did not, however, visibly touch the outer ring at all; which retained its brightness unshaded up to the point at which it was occulted by the vertex of the ball; affording, it might seem, evidence that the plane of the outer ring was sensibly raised, at that side, above the edge of the inner: moreover, the impression derived from the shape of the shadow was that the surface of the inner ring was convex, and perceptibly thicker at the middle than at either of its edges.

"I subjoin a sketch of that portion of the planet necessary to exhibit the position of the shadow; and perhaps it may be de-



sirable to add, with reference to the value of the observation, as illustrative of the quality of the vision afforded by the condition of the atmosphere, and the excellence of a speculum lately refigured for me by Mr. Lassell, that the small star of ν *Andromedæ*, also near the meridian, was readily *divided* with the power 462, the division being seen not by occasional and instantaneous glimpses, but steadily and repeatedly for three or four seconds at a time, with such distinctness that an eye used to such objects could scarcely have overlooked the duplicity. The transparency also of *Saturn's* faint ring was also satisfactorily visible, especially at the preceding limb of the ball, where it was more manifest than at the other side."

Observations of Amphitrite and Bellona made at the Observatory of Cambridge. By Professor Challis.

AMPHITRITE.

CAMBRIDGE.

1854.	Green. M.T. h m s	R.A. h m s	Par. Corr. × Δ. s	N.P.D. ° ′ ″	Par. Corr. × Δ. ″	No. of Comps.	Star.
March 6	11 48 21·8	13 15 2·08	−0·215	100 3 56.0	−7·37	13	*a*
11	11 42 48·2	13 11 54·65	−0·193	99 58 56·6	−7·41	6	*a′*
13	10 53 47·5	13 10 32·19	−0·242	99 56 12·7	−7·31	10	*b*
16	13 30 36·1	13 8 11·68		99 50 28·9	−7·55	Meridian.	

Assumed Apparent Places of the Stars.

	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″	Catalogue.
a	13 16 16·79	100 6 26·0	Bessel xiii. 256
a′	13 16 16·88	100 6 26·6	—
b	13 9 49·87	99 46 34·0	Bessel xiii. 255

BELLONA.

CAMBRIDGE.

1854.	Green. M.T. h m s	R.A. h m s	Par. Corr. × Δ. s	N.P.D. ° ′ ″	Par. Corr. × Δ. ″	No. of Comps.	Star
March 10	10 41 4·4	11 59 29·80	−0·183	81 34 50·0	−6·02	8	*c*
11	10 13 32·2	11 58 46·88	−0·213	81 25 44·9	−6·05	9	*c′*
13	12 31 37·3	11 57 13·51		81 6 51·8	−5·85	Meridian.	
16	12 17 34·9	11 54 58·49		80 40 11·8	−5·80	—	

Assumed Apparent Places of the Star.

	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″	Catalogue.
c	11 56 50·95	81 47 15·3	Bessel xi. 973
c′	11 56 50·96	81 47 15·4	—

The equatoreal observations were taken with the Northumberland telescope.

On the Influence of Climate upon the Telescopic Appearance of a Celestial Body. By James William Grant, Esq.

(*Extract of a Letter to the Editor.*)

"I send you two sketches of *Mars*, made whilst the planet was in opposition. No. 1 was an attempt made in October 1847 to depict it as seen in Calcutta through a 5-foot achromatic belonging to me, the planet being then in opposition; No. 2 I tried my

1

2

hand upon a few days ago, and it shows what I saw through the large telescope.* The atmosphere in India is very different for observation to anything I have seen here. My Indian sketch, you will observe, is actually a little world; and I doubt whether the large glass here would show it equally well, even were the weather favourable, which, I am sorry to say, hitherto it has not been.

* The telescope to which Mr. Grant refers is a refractor of 11 inches aperture and 16 feet focal length, by Mr. Ross, of Featherstone Buildings. It is the instrument which formed such a conspicuous object in the nave of the Crystal Palace during the Great Exhibition of 1851.

There is a very fine 5-foot achromatic, mounted as a finder, to my large instrument; and I tried it on *Mars*, with the same power I used when taking the Calcutta sketch; but it was very poor. I send you these particulars, that you may mention them to those who have to decide upon getting up a large instrument for a finer climate than England, as I think that different climates should be tried before the position is finally fixed upon.

"*Wester Elchies, March* 14, 1854."

Extract of a Letter from Professor Colla to Mr. Hind, dated Dec. 5, 1853.

"I have the satisfaction of informing you that on the morning of the 29th of November, I found the comet of Bruhns after its passage of the perihelion, above the star ϑ of *Serpentarius*. I employed in searching for it a refractor by Lerebours, of 4 inches aperture, with a magnifying power of 60, but it was only with the greatest difficulty that I succeeded in seizing it; for it is extremely faint, without nucleus, without trace of tail, exhibiting only, even with a magnifying power of 100, a very minute nebulosity of uniform brightness, with a feeble appearance of scintillation by intervals.

"I observed it again about 6 o'clock on the morning of the 30th of November, but even with greater difficulty, for it appeared very close to a bright telescopic star which rendered it almost insensible.

"At $5^h\,45^m$ on the morning of December 3, the sky being very clear, I found the comet again with facility. The nebulosity was more distinct, but there was no trace of nucleus, of tail, or of scintillation, even when employing a magnifying power of 100. The positions differed very little from those given by M. D'Arrest, in No. 880 of the *Astronomische Nachrichten*."

On the Orbit of the Eighth Satellite of Saturn.
By Warren De La Rue, Esq.

At one of the meetings of the Society held last year Mr. De La Rue exhibited a diagram of the apparent orbit of the eighth satellite of *Saturn* for the 16th of November, 1852, founded on his own micrometrical measurements, and those of Mr. Lassell at Malta. By a graphic process he obtained the following rough approximation to the elements of the true orbit of the satellite:—

Mean longitude on orbit, Nov. 16, 1852 ...	265°	
Semi-major axis at mean distance	514″·7	
Eccentricity	0·025	
Longitude of Perisaturnum	130°	M. Equinox, 1852·0
Longitude of ascending node	141° 20′	
Inclination to the ecliptic	18° 30′	
Sidereal revolution	$79^d\,9^h\,13^m\,12^s$	

Extract of a Letter from W. C. Bond, Esq., to the Editor.

"Perhaps I ought to have informed you earlier, that on the 28th of November I received a letter from Mr. Robt. Van Arsdale, of Newark, New Jersey, in which he says, under date of November 25th, as follows:—

"'I noticed a comet last evening in the constellation *Cassiopeiæ:* it is small, round, and bright, in a refractor of $6\frac{1}{8}$ inches aperture. Its approximate place, Nov. 25, at $6^h\ 50^m$ Newark mean solar time, was, right ascension, $2^h\ 7^m$; north declination, 60° 12'.'

"This comet was subsequently observed several times by Mr. Van Arsdale and by ourselves; and is no doubt the same seen by M. Klinkerfues on the 2d of December. Inclosed you will find our observations on this comet. Of late we have been too busily engaged in other matters, particularly in reducing zones of small stars, which we had observed by the electric method, to give much of our attention to comets.

Observations on Van Arsdale's Comet, made at the Observatory of Harvard College.

	Camb. M.S.T.	Δα, Comet − ✱	Δδ, Comet − ✱	
	h m s	m s	′ ″	
1853, Dec. 14	12 52 28·4	−1 24·24	−7 8·1	*a*
15	6 51 23·9	+1 16·74	+6 14·3	*b*
24	7 32 24·2	+0 16·63	+2 30·7	*c*
27	7 0 46·8	+0 2·15	+5 0·5	*d*
1854, Jan. 2	7 8 58·8	+0 42·50	+6 13·6	*e*

Mean Places of the Stars of Comparison referred to the Mean Equinox of the beginning of the years 1853 and 1854.

Star	α 1853·0	δ 1853·0	Authority.
	h m s	h m s	
a	1 15 17·01	+26 56 48·1	B.A.C. 409
b	1 12 10	35 59	Approximate 9th mag.
c	1 6 40	26 36	— —
d	1 5 46·42	23 48 17·7	B.A.C. 365
e	1 4 33·35	+18 57 48·0	10th mag. comp. with B.A.C. 336

"*Harvard College Observatory, Cambridge, Feb.* 16, 1854."

Extract of a Letter from M. Otto Struve to the Astronomer Royal, dated January 17, 1854.

"You will see by the enclosed paper that our Academy has resolved to propose the theory of Biela's Comet as a prize problem. Be pleased to insert our programme in the *Monthly Notices.* On that occasion it might be mentioned that all stars used formerly for comparison with the comet, and the positions of which appear not to be sufficiently ascertained, will be reobserved during the present

year with our meridian circle. A part of the work is already done."

The following is the programme alluded to by M. Otto Struve:—

Programme of a Prize for Astronomy proposed by the Imperial Academy of Sciences of St. Petersburg, at their Sitting on the 29th of March, 1853.

The disruption of the comet of Biela into two bodies apparently separated by an empty space is a unique event in the annals of astronomy. This catastrophe, which occurred in 1846, almost under the eyes of astronomers, has been followed by extraordinary changes in the aspect and brightness of the two heads. Analogous changes were again exhibited on the occasion of the return of the comet in 1852, notwithstanding the considerable augmentation of the mutual distance of the two heads. They indicate, according to all appearance, the continued existence of a mutual action of a very remarkable character. All these circumstances concur to render the comet of Biela an object of the very highest scientific interest. Further, it is to be remarked that, on the occasion of the last apparition of the comet in 1852, the observed positions have differed enormously from the ephemeris calculated beforehand by Sig. Santini, and that it is not yet decided whether the differences between theory and observation ought to be attributed solely to the imperfect nature of the calculations, or whether they have been produced by forces which, at the time of the subdivision of the comet, had effected violent alterations in the orbit. Consequently, a vigorous investigation of the orbit of the comet has appeared to the Imperial Academy of Sciences to be worthy of being proposed to the competition of astronomers and geometers, as the subject of an extraordinary prize. Considering that it is impossible, under present circumstances, to foresee all the consequences to which the researches will lead, the Academy does not think that it would be desirable to restrict too rigorously the problem proposed for solution.

The programme is accordingly comprised in the following points:—

1. The Academy demands a rigorous investigation of the elements of the orbit described by the centre of gravity of the comet of Biela; which must be founded on a minute discussion of all the observations obtained during the six apparitions observed between 1772 and 1852, respect being had to the sensible perturbations produced by the planets.

2. In case it may not be possible to represent in a satisfactory manner all the observations by means of a single orbit, it will be necessary to divide the investigation into two parts, one of which will embrace the observations from the earliest period down to the epoch of the separation of the two heads; the other will include all the observations made from that epoch down to the present time.

3. The investigation should be especially directed to the mutual relations of the two heads, in order to determine not only the posi-

tion of the centre of gravity of the two heads, but also, if it be possible, the laws according to which the alterations of their relative positions are effected.

4. The memoir for competition ought to be accompanied by an ephemeris, calculated beforehand for the next apparition of the comet in 1859.

Considering the long and troublesome calculations which the complete solution of this problem will demand, the Academy fixes the 1st of August, 1857, as the term for presenting the memoirs destined for competition. The adjudication of the prize will take place at the public sitting of the same year.

The memoirs for competition may be written in Russian, Latin, French, German, or English. They are to be addressed anonymously, "*To the Imperial Academy of Sciences of St. Petersburg*," but furnished with a motto. A sealed packet annexed bearing the same motto as the memoir will contain the name and address of the author. The memoir crowned will be published by the Academy, and the author will have fifty copies placed at his disposal. The packets of the unsuccessful memoirs will be burned, and the memoirs will be placed at the disposal of the authors.

The prize is fixed at 300 ducats of Holland.

There will be besides an *accessit* of 150 ducats of Holland.

Mr. Hind, the Foreign Secretary of the Society, has received a letter from M. C. Rümker, in which he begs the Society to accept his warmest thanks for the honour they have conferred on him in awarding him the gold medal of the Society. Dr. Lee has also received a letter from M. Rümker, in which the writer states that the Burgomaster and their High Worships the Senators and Aldermen of Hamburg, in consequence of the honour which the Royal Astronomical Society has thus conferred on the Director of their Observatory, have decreed to repair and enlarge the latter, and to mount it with a better revolving dome. They have also held out the hope of furnishing it with a larger equatoreal.

Mr. Guthrie, a gentleman residing near Bervie, N. B., wishes to call the attention of astronomers to a phenomenon which he states to have observed in the planet *Venus* a few years ago. On the occasion of the approach of the planet to its inferior conjunction, and when the crescent was very slender, Mr. Guthrie, while engaged in examining it with a Newtonian reflector of 5 inches aperture and a magnifying power of 144, was unexpectedly surprised by observing an annular fringe of light surrounding the dark side of the disk, and completing the circle which was partially formed by the outer margin of the crescent. The same appearance was observed when the planet was viewed with a Gregorian reflector of $3\frac{1}{2}$ inches aperture, magnifying 68 times. In order to assure himself beyond doubt of the reality

of the phenomenon, Mr. Guthrie directed some of his friends to examine the planet through his instruments, but without communicating to them a knowledge of the object he had in view; whereupon each of them remarked that he observed the same appearance. This luminous continuation of the cusps of the planet was observed on several consecutive nights, the atmosphere having been very favourable for observation. It increased in visibility as the crescent waned. The breadth was considerable: neither the interior nor the exterior side of it was sharply defined.

Mr. Guthrie states that the instruments with which the planet was examined were both of excellent quality. The fringe was better defined in the larger instrument than in the smaller; being of a nebulous appearance, it was more distinctly visible in the smaller than in the larger instrument.

Mr. Guthrie ascribes the luminous appearance above mentioned to the existence of an atmosphere about *Venus*. He was prevented by the unfavourable state of the weather from verifying the existence of the phenomenon on the occasion of the late inferior conjunction of the planet. It is well known that Schroëter observed a similar continuation of the cusps of the planet, although not so extensive as to form with the crescent a complete circle of light.

Astronomical Observations made at the Observatory of Cambridge. By the Rev. James Challis, M.A., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge, and late Fellow of Trinity College. Vol. xvii. for the years 1846–7–8. 4to. Cambridge, 1854.

This volume contains the meridian observations with the transit and mural circle, and the occultations of stars by the moon, for the years 1846–7–8. The observations of comets and planets with the Northumberland equatoreal have been reserved for a separate publication.

The meridian observations of 1846 are confined to stars, with the exception of observations of the planet *Neptune* in the last quarter. In 1847 an effort was made to resume meridian observations of bodies of the solar system, and, accordingly, in that year the Sun, the Moon, *Jupiter*, *Saturn*, and *Uranus*, and the new planets, *Neptune*, *Flora*, *Iris*, and *Astræa*, were observed. But the pressure of the work with the Northumberland equatoreal in observing planets and comets rendered it necessary, after February 1848, to restrict the meridian observations of moving bodies to the newly-discovered planets. Of these *Flora*, *Metis*, *Hebe*, *Astræa*, and *Neptune*, were observed in 1848.

A large number of the stars observed in the three years were used for comparison in equatoreal observations. The rest are double stars in Struve's catalogues, moon-culminating stars, occultation-stars, stars whose places are imperfectly determined in the British Association Catalogue, and the stars observed for instrumental corrections.

*Determination of the Difference of Longitude between Frankfort-on-the Maine and Berlin by Means of Galvanic Signals.** By M. Encke.

The two extreme stations were Paul's Tower, Frankfort, and the new Observatory, Berlin. M. Encke and Dr. Brünnow observed at the latter station, and Dr. Lorey at the former. The telegraph apparatus used is Morse's. It was agreed that Dr. Lorey should decide in every case upon the day for making the experiments, in order that he might be enabled to determine the time with accuracy. He was then to announce by a signal that the experiments were about to commence, and after this note of preparation he was to make a series of signals during the next ten minutes, it being arranged that he was to make a new signal about the beginning of each successive minute. M. Encke observed the corresponding time at the Telegraphic Office, Berlin, upon a chronometer by Tiede, which was regulated according to mean time; Dr. Brünnow made similar observations with a chronometer by Tiede regulated to sidereal time. The ten signals being observed, Dr. Brünnow gave a signal from Berlin to announce that a new set of experiments was about to commence, and this was followed by ten successive signals as before. The time was observed in this case by Dr. Lorey at Frankfort, and by M. Encke at Berlin. Since the time of each approaching signal was known within a few seconds, the observer could wait for its actual arrival with unremitting attention.† In this way all went on quickly and without any disturbance. Only on the second day was the last signal imperfectly observed. M. Encke then gives the details of the observations. The following are the final results for the longitude of Frankfort west of Berlin:—

Signals from Frankfort.

	m s	m s
Aug. 12	18 51·99 E	18 51·72 B
28	51·71	51·85
	18 51·75 E	18 51·79 B

Mean $18^m\ 51^s{\cdot}77$

Signals from Berlin.

	m s	m s
Aug. 12	18 51·57 E	18 51·93 B
28	51·91	52·13
	18 51·74 E	18 52·03 B

Mean $18^m\ 51^s{\cdot}89$

or, on the whole, a mean difference of longitude amounting to $18^m\ 51^s{\cdot}83$. The difference of the two results cannot arise from the circumstance that the transmission of the signal is not instantaneous, since, during the interval of transmission, the Frankfort signals ought to have arrived too late at Berlin, in which case the

* *Ast. Nach.* No. 889. † Mit gespannter aufmerksamkeit.

difference of longitude should have turned out too great, while again the Berlin signals, by arriving too late at Frankfort, should have indicated the difference of longitude to be too small. In reality, however, the Berlin signals made the longitude west *greater* than its true value. These two experiments, consequently, indicate that, as regards the means of observation herein employed, the velocity of the electric current is insensible for the distance between Frankfort and Berlin, a distance which, taking into consideration the circuitous route of the telegraph, certainly exceeds 60 German miles.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIV. *April* 12, 1854. No. 6.

REV. RICHARD SHEEPSHANKS in the Chair.

Wm. Huggins, Esq. 97 Gracechurch Street; and
John Hamilton, Esq. Aylesbury,

were balloted for and duly elected Fellows of the Society.

Professor Hansen, the Editor of the *Astronomische Nachrichten*, lately informed the Astronomer Royal that it had been found necessary to discontinue the transmission of the numbers of the *Astronomische Nachrichten* to England by Post, in consequence of the heavy charge made on them by the subordinate officers of the Post-Office, who insisted on considering them as letters, and requiring the letter-postage. The Astronomer Royal referred the matter to the Secretary of the Post-Office, and has the pleasure to communicate to the members of the Royal Astronomical Society the answer which he has received, as follows:—

"*General Post-Office, 2d May*, 1854.

"Sir,—With reference to your communication of the 20th ultimo, I beg leave to inform you that the publication in question is not liable to any charge in this country.

"If you have in your possession a copy which has been charged with postage as stated, I shall be glad if you will forward it to this office for inspection.—I am, Sir, your obedient, humble Servant,

"J. P. GODBY,
"For the Secretary.

"*Professor Airy,*
"*Royal Observatory, Greenwich.*"

The Astronomer Royal trusts that after this clear statement no difficulty will be experienced by any English astronomer in receiving the *Astronomische Nachrichten* without charge. If, however, the letter-postage be demanded, it will be best to receive the number of the *Astronomische Nachrichten*, and to pay the postage, and then to communicate the circumstances to the Secretary of the Post-office, referring to the assurance contained in the letter above.

Observations of Comet II., 1854, made with the Northumberland Equatoreal at the Cambridge Observatory.

1854.	Green. M.T. h m s	R.A. h m s	Par. Corr. × Δ. s	N.P.D. ° ′ ″	Par. Corr. × Δ. ″	No. of Comps.	Star
March 29	8 12 55·6	1 6 27·7	.....	70 12 35	.....	1	
30	7 41 46·1	1 21 42·37	+0·363	70 21 44·3	−6·75	7	*a*
31	7 42 15·6	1 37 3·57	+0·364	70 44 19·4	−6·68	5	*b*
April 1	7 56 40·8	1 52 8·24	+0·364	71 18 0·9	−6·72	6	*c*
3	7 57 32·8	2 20 17·58	+0·365	72 49 37·1	−6·61	7	*d*
4	7 50 20·8	2 33 16·97	+0·365	73 43 56·2	−6·53	2	*e*
5	8 16 7·4	2 45 48·03	+0·362	74 43 0·4	−6·67	2	*f*
	8 25 49·1	2 45 52·44	+0·360	74 43 15·8	−6·72	1	*g*
6	8 5 10·4	2 57 15·39	+0·362	75 42 27·6	−6·58	3	*h*
7	7 56 16·8	3 8 1·81	+0·361	76 43 25·3	−6·52	10	*i*
8	8 9 13·2	3 18 14·83	+0·359	77 44 52·0	−6·59	12	*k*

The position of March 29 is very rough, the comet being near setting.

Assumed Apparent Places of the Stars.

	Apparent R.A. h m s	Apparent N.P.D. ° ′ ″	Catalogue.
a	1 18 28·23	70 41 24·4	B.A.C. 430
b	1 34 33·24	70 26 37·8	B.A.C. 523
c	1 54 17·51	71 19 7·1	Rümker 512
d	2 22 47·31	72 56 43·6	Green. 12-yr. Catalogue
e	2 37 34·70	73 35 57·3	H.C. 5099
f	2 49 18·96	74 17 48·3	Rümker 751
g	2 50 7·02	74 35 46·4	Rümker 755
h	2 58 32·76	75 47 37·7	Bessel ii. 1037
i	3 8 20·34	76 41 30·6	Bessel iii. 139
k	3 19 15·22	77 46 37·6	Rümker 858

The places of the Catalogues are adopted.

J. CHALLIS.

Observations of the Comet made at the Observatory of Redhill.

1854.	Green. M.T. h m s	R.A. h m s	Log $\frac{p}{P}$	N.P.D. ° ′ ″	Log $\frac{q}{P}$	Stars.
Mar. 29	8 0 13·8	1 6 20·84	+8·620	70 12 40·7	−9·9049	*a*,
30	7 57 12·6	1 21 53·18	8·629	70 22 2·4	9·8982	*b*, *c*,
31	7 32 8·0	1 36 56·05	8·643	70 44 4·5	9·8807	*d*, *e*,
	7 45 4·7	1 37 5·17	8·639	70 44 16·8	9·8871	*e*, *f*,
April 1	7 28 8·0	1 51 51·08	8·644	71 17 11·8	9·8741	*g*,
	8 11 4·0	1 52 17·64	8·631	71 18 27·9	9·8950	*h*, *i*,
3	8 18 36·1	2 20 30·62	+8·633	72 59 8·8	−9·8907	*k*.

Corrected for refraction.

Assumed Mean Places of the Stars, 1854·0.

		h m s	° ′ ″
a	B.A.C. 348	1 3 36·90	69 44 35·4
b	Bessel Z. 200	1 15 32·28	70 17 40·5
c	— —	1 26 52·98	70 34 28·7
d	B.A.C. 523	1 34 34·87	70 26 32·1
e	— 533	1 36 57·70	70 38 50·9
f	Lalande 3188	1 36 55·81	70 52 38·4
g	B.A.C. (572...3)	1 45 31·53	71 25 21·2
h	Bessel Z. 200	1 48 16·38	70 58 59·3
i	— 332	1 54 19·45	71 19 2·0
k	B.A.C. 771	2 22 48·93	72 56 35·9

"The above observations were taken by noting the times of passage for comet and star over two bars at right angles, whose inclination to a parallel ($\alpha = 44° 47'$) was deduced from our observations of the sun, according to the method explained in the last *Monthly Notice*.

"Corrections for hourly motion of the comet while passing from one bar to the other were applied, by adding to its R.A. the quantity

$$-\tfrac{1}{2}(P_b - P_a) \cdot \varkappa,$$

and by multiplying the quantity C D for the comet by the additional factor

$$\frac{3600}{3600 + I} \cdot (1 - \varkappa^2)$$

where I = hourly motion in R.A., I′ in N.P.D., and $\varkappa = \dfrac{I'}{3600 \cdot 15 \cdot \sin \delta}$.

"The hourly motions were taken from an ephemeris computed for the purpose from the elements given in the last *Notice*. The perihelion passage is there incorrect from a fault of my own: it should have been March $24^{d} \cdot 12742$.

"N.B. The observation on March 29 was taken at nearly 87° zenith distance. R. C. Carrington.

"1854, *April* 28."

Extract of a Letter from Prof. Argelander to the Astronomer Royal, dated April 7, 1854, accompanying two sets of Elements, and an Ephemeris of the Comet now visible.

"Without doubt you will before this time have found the fine comet, which is now visible in the evening twilight. But under the uncertainty whether the weather has been sufficiently favourable to enable you to obtain a competent number of observations for the determination of an orbit, I take the liberty to forward to you two sets of elements which have been computed, the first by M. Schönfeld, the second by M. Krüger and M. Förster, together with an ephemeris based on the latter elements.* The near agreement of

* A few copies of this ephemeris were printed off and distributed among the various observers in the country.

the two sets warrants a favourable judgment on their correctness, although the first observation by Luther on which they are based, is not very certain. Still we have preferred this to our own observation, since we were not able to observe the comet, which was discovered by M. Krüger on March 30, in a satisfactory manner, till April 2. On the preceding days clouds permitted only some rough estimations of its place.

Elements of the Second Comet of 1854.

	By Schönfeld.	By Krüger and Förster.
	From Bilk Obs. of April 1, and Bonn Obs. of April 2 and 3.	From Bilk Obs. of April 1, and Bonn Obs. of April 3 and 5.
	T. Mar. 24·0029, M.T. Berlin.	Mar. 24·0336, M.T. Berlin.
	° ′ ″	° ′ ″
π	215 7 33	214 37 21
Ω	316 2 21	315 42 0
i	83 53 40	84 19 44
log q	9·43873	9·43904
	Motion retrograde.	

"This comet is *not* identical with that of Brorsen, discovered on March 16, in R.A. $2^h\ 30^m\ 12^s$, and Decl. $+1°\ 11'·2$, since the present comet, at the time of Brorsen's observation, was in R.A. $22^h\ 57^m$, and Decl. $+5°$."

Extract of a Letter from M. Laugier to Mr. Hind.

"Allow me to send you the observations of the new comet which I have made conjointly with MM. Charles Mathieu and Ernest Liouville.

"The place of observation, situate at No. 183 Rue de Vaugiraud, Paris, is 2^s to the west, and 24″ north of the observatory. The equatoreal employed is by M. Brunnen, artist of the Board of Longitudes, who placed it at our disposal. The whole of the observations are corrected for refraction.

1854.	Paris M.T. h m s	Comet's R.A. h m s	Comet's Decl. ° ′ ″
March 31	7 21 43	1 36 44·14	+19 16 20·3
April 1	7 19 11	1 51 39·65	18 43 8·6
2	7 29 28	2 6 11·75	18 0 31·2
3	7 54 16	2 20 11·80	17 10 41·0
5	7 46 50	2 46 13·24	15 15 6·3
6	7 51 18	2 57 4·54	14 17 7·8
7	7 39 56	3 7 49·95	13 18 0·4
8	7 40 13	3 17 59·68	12 16 29·0
9	7 48 56	3 27 18·74	11 15 32·9
10	7 55 19	3 36 13·10	10 15 43·4
13	7 55 30	3 59 45·06	7 28 14·8
15	8 13 27	4 13 3·67	+5 45 17·6

Elements and Ephemeris of the Second Comet of 1854.

By J. R. Hind, Esq.

"From three observations of the new comet, taken at Mr. Bishop's Observatory on the evenings of March 29th and April 1st and 4th, I have calculated the following elements of its orbit:—

Perihelion Passage, 1854, March 24.

Longitude of Perihelion	213° 47′ 32″	True Eq^{x} April 0.
Ascending Node	315 27 53	
Inclination.....	82 23 54	
Log of least Distance	9·4427392	

Motion retrograde.

"These elements have some slight resemblance to those of the second comet of 1799, observed by Mechain at Paris about Christmas in that year; but the similarity is not sufficiently great to justify a suspicion of identity.

"On the 29th of March, when the comet was generally observed throughout this country, its appearance was finer than that of the one visible in August last; the nucleus was brighter than stars of the second magnitude, and the tail could be traced through a space of 4° or upwards. At the beginning of the present month the tail was very sensibly curved.

"The following places of the comet have been computed from the above elements:—

At Greenwich Mean Noon.

1854.	Comet's R.A. h m s	Comet's N.P.D. ° ′	Log Δ.	Log *r*.
April 12	3 50 2	81 20·2	9·9941	9·7947
13	3 57 27	82 14·8		
14	4 4 25	83 7·4	0·0133	9·8254
15	4 10 56	83 57·9		
16	4 17 5	84 46·3	0·0326	9·8539
17	4 22 53	85 32·6		
18	4 28 21	86 16·9	0·0519	9·8803
19	4 33 32	86 59·2		
20	4 38 26	87 39·6	0·0709	9·9049
21	4 43 6	88 18·2		
22	4 47 32	88 55·2	0·0893	9·9279
23	4 51 46	89 30·6		
24	4 55 49	90 4·5	0·1071	9·9494
25	4 59 41	90 37·0		
26	5 3 23	91 8·0	0·1243	9·9667
May 6	5 33 52	95 22·1	0·2009	0·0558

"The co-ordinate constants (true equinox, April 15) are as subjoined:—

$$x = r \cdot [9{\cdot}8566309] \cdot \sin(340^\circ\ 54'\ 43'' + v)$$
$$y = r \cdot [9{\cdot}9049116] \cdot \sin(205\ \ 7\ \ 0 + v)$$
$$z = r \cdot [9{\cdot}9615897] \cdot \sin(96\ \ 5\ 14 + v)$$

"*Mr. Bishop's Observatory*, 1854, *April* 12."

Observations of Comet II., 1854, *made at Markree Observatory.*

Green. M.T. 1854.	R.A. h m s		Decl. ° ′ ″	
April 14·380856	4 6 56·54	+[9·5281]+Δ	6 33 11·4	+[0·8370]÷Δ
15·371142	4 13 16·36	[9·5262]	5 44 2·4	[0·8353]

Mean Places of Compared Stars, 1854·0.

	h m s	° ′ ″	
April 14	4 1 38·765	6 20 23·3	Weisse 4^h 2
15	4 12 54·49	5 46 42·85	Rümker, 4^h 1160

The result on April 14 is from three observations by Mr. Cooper, that on April 15th is from three by Mr. Cooper and three by Mr. Graham.

The respective results on April 15, reduced by my ephemeris to the mean time, are,—

h m s	° ′ ″	
4 13 16·45	5 44 3·27	E. I. C.
4 13 16·28	5 44 1·62	A. G.

My first rough orbit still gives the comet's place with very tolerable accuracy.

Last night (April 17th) low clouds covered the comet.

A. GRAHAM.

Markree Observatory, April 18, 1854.

Elements of Comet II. 1854.

Professor Littrow has forwarded the following elements of the comet, which he very obligingly accompanied with an ephemeris, extending from April 1 to May 4; but, as the latter does not differ materially from similar communications received from other sources, and which had already been in type, it has not been deemed necessary to publish it. Prof. Littrow remarks that the comet has no connexion either with the bright nebulous object seen by M. Brorsen on the 16th of March, or with the comet of 1556, and seems to be new.

Elements of the Comet computed by M. Hornstein.

Perihelion Passage, 1854, March 24·06022 M.T. Berlin.

Longitude of Perihelion	213° 47′ 55·4	App. Equinox, 1854, April 3.
Longitude of Node	315 26 49·8	
Inclination	82 22 40·9	
Log Perihelion Distance	9·4425544	

Motion retrograde.

These elements are computed from four observations of Vienna of the 1st, 2d, 4th, and 5th of April. The following are the outstanding deviations from the observations:—

	Obs.—Cal.	
	$d.l$	$d.\lambda$
	″	″
April 1	0·0	0·0
2	−4·9	+6·1
4	+8·0	−2·1
5	0·0	0·0

Observations of Comet II. 1854. By T. W. Burr, Esq.

"Although the Society will no doubt be furnished with numerous observations of the new comet, far superior in accuracy to any I can offer, I am nevertheless induced to forward those I have made, in case they may by chance be useful in filling up a vacancy in the series, and thus aid in completing our knowledge of so interesting a body.

1854.	Green. M.T. h m s	R.A. h m s	N. Decl. ° ′	Bar.	Ther.
March 30	7 33 25	1 21 36·4	*19 40	30·28	54
	7 44 6	1 21 55·4	...	...	...
31	7 56 10	1 37 25·4	*19 22	30·37	62
	8 2 19	1 37 31·4	...	...	...
April 1	7 29 18	1 51 52·3	*19 12	30·28	60
	7 31 49	1 51 55·3	...	...	...
	7 49 51	1 52 4·3	...	...	...
3	7 2 25	2 20 4·4	17 12	30·52	56
	7 33 46	2 20 22·4	...	...	...
	7 54 28	2 20 36·4	...	...	...
4	7 26 10	2 33 0·3	16 18	30·42	...
	7 32 4	2 33 7·8	...	...	...
	8 12 29	2 33 39·3	...	...	54
5	7 21 15	2 45 28·8	15 17	30·36	58
	7 36 46	2 45 35·8	...	...	...
6	7 28 51	2 57 6·3	14 20	30·38	...
	7 47 20	2 57 19·3	...	...	...
	8 18 29	2 57 35·3	...	...	56
9	7 54 31	3 27 52·4	11 17	30·21	58
	8 24 0	3 28 0·4	...	...	56

* "These observations in declination are probably incorrect, as on the 2d of April I discovered that the verniers of the circle were loose and shifting. They were then properly fixed and adjusted."

March 30. Very fine with naked eye.
31. Seen with difficulty among clouds.
April 1. Fine views without telescope.
3, 9. Not visible to naked eye during observations.

"The observations are not corrected for refraction, which would of course act powerfully on a body so near the horizon, nor for any other cause of alteration in position except instrumental errors. I have therefore added the atmospheric pressure and temperature for each evening. The barometer is about 85 feet above Trinity high-water mark. Approximate latitude and longitude, 51° 33′ 45″ N., and 23ˢ·8 W.

"The instrument used was the equatoreal mentioned in a previous notice of mine respecting *Saturn's* rings. It is by Ross, of 4 feet focal length, and $3\frac{3}{8}$ inches aperture, and has a Fraunhofer mounting. It reads only to 4ˢ in right ascension, and single minutes in declination, and the observations may therefore vary within those limits, though sometimes careful estimations of smaller distances have been attempted. The clock error was determined by several transits each evening prior to and during the observations on the comet.

"Having no catalogue at hand but the *Nautical Almanac*, and the daylight being generally so strong as to prevent small stars in the neighbourhood being visible, I have been compelled to use *α Arietis* during the first three days' observations, and the same star and *η Tauri* in later ones, as stars of comparison.

"On the 3d April the comet was found in the telescope at 6ʰ 55ᵐ G.M.T., when the sun had only set about 20 minutes, and the light was therefore very strong. The planet *Saturn*, which was looked at for comparison, was distinguishable, with about the same facility as the comet, both being faint objects, while *Aldebaran* was very bright and easily seen.

"The nucleus of the comet was throughout of a golden tint, apparently 10″ or 12″ in diameter, and enveloped in the coma which extended round it on the side nearest the sun, while the tail stretched in the opposite direction, and was continued with nearly parallel sides and of a brush-like character of rays to some distance. It has lately become somewhat more divergent in appearance, but has not to my eye assumed a curvature at all. On the 30th March the tail was estimated by the unassisted eye to be about 2° in length, and upon measuring it by means of the declination circle it was found to be quite $1°\frac{5}{6}$. On the 1st April only 1° could be thus seen, but the evening was lighter and the moonlight stronger. The same causes have conspired still further to diminish the comet's brilliancy and the length of the tail, and on the 4th instant only 15′ could be traced, but besides these causes of apparent diminution, the intrinsic brightness of the nucleus is really much impaired, as is shown by comparison with stars which it formerly equalled or surpassed in lustre. This was well proved on the 9th April, when *α Arietis* was visible some time before the

comet could be discovered, although the star was in much the more unfavourable position with regard to the twilight."

Extract of a Letter from J. C. Adams, Esq., to the Editor.

"Probably you will have plenty of elements of the comet which is now starring it, nevertheless I may mention the following, which I deduced from Professor Challis's observations on March 30, April 1, 3. A comparison of these elements with an observation on April 7, gave an error of only 10″ in longitude, and nothing in latitude, so that they are probably not far from the truth.

Perihelion Passage, March 24·01221, G.M.T.

	°	′	″
Longitude of Perihelion	213	51	32
— Ascending Node	315	29	52
Inclination	82	34	28
Log Perihelion Distance	9·4426170		

Motion retrograde.

Note on the Zodiacal Light, the Companion of Procyon, and Sirius. By the Rev. T. W. Webb.

"Since my former communication on the subject of the zodiacal light, I have noticed two instances of it which offered appearances somewhat unusual.

"1853, Dec. 30. As soon as twilight had disappeared, the zodiacal light was faintly but very evidently perceived. Had it been more distinct, I thought it would have presented a very curious appearance; but its want of brightness rendered me less certain than I could have wished. Its general character, however, seemed to be that of two or three slender tapering streams, forming altogether but a narrow cone, much fainter than the galaxy, though in a beautifully clear evening, and extending towards, if not to, the stars in *Aries*.

"1854, Feb. 25. About $8^h\ 15^m$ there was a fine bright zodiacal light, passing between the stars of *Aries*, and extending towards, or perhaps reaching as far as *Saturn*; the termination, however, was very uncertain. It was very bright as compared with the galaxy, and did not appear double or divided into streams. I once or twice fancied that the south edge was best defined, but could not satisfy myself of the fact; I had, however, a stronger suspicion that the whole beam of light was a little concave towards the north pole.

"I regret to find that the observation of the companion of *Procyon*, which appeared in vol. xiii., No. 9, of the *Monthly Notices*, is of no value. I have since remarked that the

star is surrounded by several small attendants, and it does not appear to which of them the observation in question may have referred.

"However strong may be the evidence in favour of a change in the colour of *Sirius*, I am not aware that any suspicion has been entertained of its light being variable. I cannot, however, divest myself of an impression that it has been more brilliant during the late winter than in the preceding one, or perhaps for several recent seasons; and more like what I seem to recollect it many years ago.

"*Tretire, March* 16, 1854."

Extract of a Letter from W. Lassell, Esq., to the Editor.

"In the *Monthly Notice* for March I observe some remarks, with two engravings of the planet *Mars*, from J. W. Grant, Esq., tending to show the great superiority of the climate of Calcutta over that of England. I can well believe that to be the case, but on reference to the circumstances of the last two oppositions of the planet, I find them so different that the striking inferiority of the English picture must by no means be laid entirely to the account of the English telescope or climate.

"At the opposition of *Mars* in 1847, its distance was only 43 millions of miles, and its apparent diameter nearly 20″; while at the opposition of this year he was 64 millions of miles off, and subtended an angle of only about 13″. Again, the meridian altitude at Calcutta, at the first epoch, would be about 81°, and at Greenwich at the latter epoch the altitude would be only about 51°. I do not consider that in a tolerable state of atmosphere the difference between 50° and 80° of altitude would very materially affect the vision; but the greater proximity of the planet in 1847 would tell most strikingly, and might lead to a very erroneous judgment of the telescopes pointed to it under circumstances so widely different. There is also another condition which should not be overlooked. There is generally a certain quantity of light with which a telescopic object is best seen; the phenomena of the disc of *Mars*, especially, require this condition, and it is, I think, very possible, that if the power applied to the large telescope was not very greatly superior to that of the small one, the minute lineaments visible in the latter might be quite drowned in the flood of light afforded by the large telescope. It is, I believe, a good maxim in scrutinising the minutiæ of planets, to employ pretty high powers, with only such an amount of light as is sufficient adequately to stimulate the eye; and by this means the effect of atmospherical disturbance (generally in some proportion to the aperture) is reduced to a minimum.

"*Starfield, 25th April*, 1854."

Occultation of 42 Leonis, of 3579 B.A.C., of i Leonis, and of the Planet Mars, observed at Ashurst. By Robert Snow, Esq.

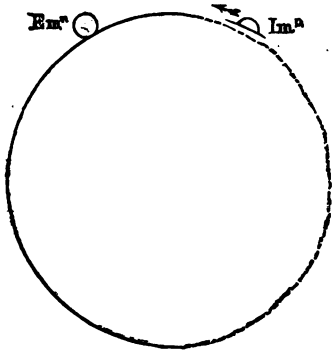
" 51° 15′ 58″ N. Lat. 1ᵐ 10ˢ W. Long.

"(1.) 42 *Leonis.* Immersion at $7^h\ 26^m\ 53^s{\cdot}6$, corrected sidereal time for the place. The moon being over thirteen days old, and excessively bright, gave an uncertainty to the observation, though the immersion took place at the dark limb. The emersion took place at the bright limb, but could not be observed; for the star was quite imperceptible, until it had quitted the moon's edge for some little distance. The definition of bright stars was far from good all the early part of the evening.

"(2.) 3579 B.A.C. Immersion at $11^h\ 48^m\ 34^s{\cdot}6$, corrected sidereal time. Well seen, and well observed. Again, the emersion could not be observed on account of the star being overwhelmed in the moon's splendour.

"(3.) *i Leonis* also approached the moon's edge very nearly, about the time set down in the *Nautical Almanac.*

"(4.) The planet *Mars* and the moon, agreeably to the time predicted, being now not far asunder, the telescope (a 5-foot equatoreal by Simms, of 4 inches aperture) was turned on the planet, and different magnifying powers tried; but the planet was not seen well with powers higher than 76; which, accordingly, was used in the observation. The state of the air, however, seemed to be gradually improving; for, although the planet now and then waved and fluttered, it might almost be said, like a pennon, yet again its roundness kept returning very satisfactorily. It was of almost precisely the same colour as the moon; and in the most favourable moments I could not help comparing it to a spangle on the face of the sky. When first the planet began to be hidden by the moon's dark edge, then its beautiful definition became disturbed, and I imagined that a violet light, not perceived before, was thrown from it, towards the left hand, in the direction of the little arrow drawn in the accompanying figure. However, I am inclined to attribute this appearance rather to a fatigued and nervous state of the eye, or to the caprice of our own atmosphere, than to the effects of the atmospheres (if such exist) of the moon, or the planet *Mars.* For the above confused impressions only lasted for a few seconds; and when the planet was rather more than half hidden, its previous distinct sphericity seemed to [illegible] while it was slowly and solemnly vanishing, it gave [illegible] the notion of its being the summit of a lunar



gradually away. For there were two pointed illuminated summits, standing out at the moon's ragged edge hard by; and the brightness and tint of the last fraction of the planet was precisely the same as theirs.

"The immersion began at $14^h\ 33^m\ 36^s$ corrected sidereal time, and, as stated above, was not observed to a certainty. The planet was half hidden at $14^h\ 33^m\ 56^s{\cdot}6$, corrected sidereal time, and was quite hidden, and well seen to the last speck, at $14^h\ 34^m\ 36^s{\cdot}6$, corrected sidereal time.

"Although the above observations give 60^s for the duration of the immersion, I should be inclined to say that perhaps 70^s would be nearer the truth.

"At the emersion, I never remember a more beautiful sight than the planet rising majestically from the moon's bright edge, without the slightest distortion from its circular form. It was now quite steady and round; and the idea of the silvery *spangle* returned stronger than before. Not the slightest perceptible change of shape took place in the planet during the time occupied in quitting the moon's bright edge. Nothing whatever was to be seen of the two bodies clinging together, as it were, by threads of light; nothing of the pear-shaped appearance often recorded as put on by planets under similar circumstances. The whole phenomenon was undisturbed, and clear and sharp as a delicate engraving.

"*Mars* began to reappear at $14^h\ 58^m\ 1^s$, corrected sidereal time, at the bright limb of the moon, and just cleared it at $14^h\ 58^m\ 57^s{\cdot}6$, thus giving 56^s for the time occupied in the re-appearance.

"This last observation was perfectly satisfactory. From the observations of both immersion and emersion, the planet was hidden by the moon for $24^m\ 31^s{\cdot}5$. It may be added that the night was quite cloudless, and that the clock-error was very nicely determined. The clock is both visible and audible from the equatoreal, as well as from the transit instrument, which has always been found to be a great practical convenience.

"*March* 12, 1854."

Occultation of s Geminorum.

The immersion of *s Geminorum* took place at the moon's dark limb, at $13^h\ 43^m\ 29^s$, corrected sidereal time for the place. The star did not vanish quite instantaneously, but seemed suddenly to become of half its brightness or magnitude, and in the next moment was totally gone. The emersion took place at the moon's bright limb, at $13^h\ 15^m\ 42^s$, corrected sidereal time. The star rose almost with a small planetary disk, so to speak, and occupied about 5^s before it was quite detached from the limb.

Ashurst, April 4th, 1854.

Occultation of the Planet Mars observed at Islington.
By Warren De La Rue, Esq.*

"March 12th, 1854, observed the occultation of *Mars* by the moon with my 13-inch reflector, and with a power of 150.

	h	m	s	
The first contact took place at	14	33	19·5	Sidereal Time.
Complete eclipse	14	34	20	

"On the instant of contact the dark limb of the moon became distinctly visible. The eclipse of the planet was gradual, and afforded no indication of inflexion of light: the event was observed until it could have occupied only a very small fraction of a second of arc in breadth, as the atmosphere was beautifully steady at the time. The epoch of emersion was lost."

Extract of a Letter from J. Hippisley, Esq., to the Editor.

"Subsequently to the date at which I wrote to you, being at Mr. Dawes', in company with Mr. Lassell, we observed *Saturn*, not, however, under more than very *moderately good* atmospheric condition. *I saw* nothing to alter my impression of the configuration or position of the shadow; but MM. Lassell and Dawes both *thought* it did touch the outer ring, giving, however, respectively, considerably different estimates of the amount.

"Inclined to distrust my own sight when it did not agree with two observers of such eminence, I was much inclined to write to you to modify the terms of my memorandum, but a *repeated* view of the appearance again at my own observatory so entirely confirmed me, that I determined to let the memorandum stand, as it *was*, right or wrong, undoubtedly what *I did* see. However, I set about computing the positions of the shadow geometrically, regard being had to the relative positions of sun and earth to the plane of the ring; and I found, assisted by a *solid model*, which carefully represented the measurements of the planet, and was fixed on the polar axis of my telescope, so as to give it with tolerable accuracy, the necessary inclination to the sunshine, that the shadow would (on the supposition that the rings *were in one plane*) reach *half way across the outer ring*; but that this part of the shadow would be so nearly *occulted* by the ball to an observer on the earth, that the minute portion which would remain would probably be insufficient to be visible under any conditions of atmosphere to be ever hoped for.

"I made due allowance in the model for the difference of polar and equatoreal diameters of the planet, which is material, as the consequence is a configuration of the curves of the *shadow*, and of

* Mr. De La Rue estimates his observatory to be 20ˢ west of Greenwich.

the *occultation*, favourable to the invisibility of the shadow at the epoch (February 12, 1854). I inclose a sketch of these curves to explain the position of the minute portion of shadow, not included within the curve of occultation. If this drawing be held edgeways, as the rings are now seen, a simple inspection will show that, when foreshortened, that portion becomes scarcely perceptible even *with the paper in one's hands*, instead of an object under high power in the telescope; and if the plane of the outer ring be in the least raised above the inner and the equator of the planet, it would disappear altogether *geometrically* as well as *optically*.

"I may just add that, since his return to Liverpool, Mr. Lassell has written to me; and in his letter observes that *he believes* he sees the outer ring, as I describe, free from shadow, though not with sufficient certainty to speak with entire confidence."

"*Ston Easton, April* 6, 1854."

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*Account of a Portable Observatory erected by Thomas Dell, Esq.**

"The model of my small *portable* observatory requires but very few words of explanation, and it is only in the hope that a short account of it may be of use to persons who have but small means and space at their disposal that I venture to offer it.

"The erection consists of two apartments, with a door between them. The one contains the transit instrument, which, for the purpose of economising space, is placed very much on one side; and the other the equatoreal and clock. The dimensions of the transit-room, 7 feet 6 inches in length by 5 feet 6 inches in width, and 6 feet 6 inches in height. The equatoreal-room is 10 feet in diameter, the walls being 7 feet 3 inches high, surrounded by a conical roof, which revolves on rollers let into the oak curbing, to which the walls are attached, and being very light is easily turned with one hand. The piers which carry the polar axis are of well-seasoned oak, 1 foot square, above the ground, and set in concrete to a depth of 6 feet beneath it. These I find quite steady enough, (they being, of course, perfectly independent of the floor and wall) for the telescope they carry, it having a focal length of 6 feet and $4\frac{1}{4}$ inches aperture. The polar axis consists of two pieces of oak, about 7 feet long, morticed at the top and bottom to two cross pieces, to which the metal pins, on which the whole turns, are attached, and after being put together, were accurately turned in a lathe.

"The telescope moves on an axis in the manner of a transit instrument in the centre of the axis, and thus no counterpoise is required. The whole of the building, with the exception of the curbing, which is oak, is of deal, half an inch thick; the frame-

* A model of this observatory was exhibited at the last meeting of the Society.

work resting on a foundation of brick, in which apertures are left, allowing a free circulation of air beneath the floor. The whole is *screwed* together, so that it may easily be taken to pieces; indeed, having had occasion to change my residence twice, I have removed it with me. The roof is covered with galvanised iron, and the whole is painted on the outside and varnished within. The entire cost of the observatory amounts to the sum of 37*l.* 10*s.* 5*d.*

"*March 28th*, 1854."

Observations of Comet II., 1854, *by Lieut. James S. Goodenough, H.M.S. Centaur.*

(*Communicated by the Astronomer Royal.*)

April 7th, 1854.

At Porto Grande, Island of St. Vincent, C. Verds.

Lat. 16° 51′ N.; Long. 25° 4′ W.

Greenwich Mean Time	$9^h\ 27^m\ 24^s$
Obs. Dist. from *Aldebaran*	19° 13′ 0″
Altitude of *Aldebaran*	27 30 0
Altitude of Comet	8 15 0

There was only time for a single observation, as the comet was low, and shortly after was hid by clouds.

Index Corr. + 20″; Height of Eye, 18 feet.

April 8th, at Sea.

Lat. 17° 20′ N.; Long. 24° 30′ W.

Greenwich Mean Time	$9^h\ 12^m\ 39^s$
Obs. Dist. from *Sirius*	57° 5′ 40″
Obs. Dist. from *Capella*	40 33 0
Obs. Alt. of *Sirius*	44 20 0
Obs. Alt. of *Capella*	37 15 0
Obs. Alt. of Comet	12 30 0

Index Corr. + 20″; Height of Eye, 18 feet.

April 9th, at Sea.

Lat. 20° 12′ N.; Long. 23° 15′ W.

Greenwich Mean Time	$8^h\ 58^m\ 20^s$
Obs. Dist. from *Sirius*	54° 36′ 0″
Obs. Dist. from *Capella*	40 21 40
Obs. Alt. of *Sirius*	43 25 0
Obs. Alt. of *Capella*	40 20 0
Obs. Alt. of Comet	16 15 0

Index Corr. + 20″; Height of Eye, 18 feet.

April 10th, at Sea.

Lat. 22° 45′ N.; Long. 22° 12′ W.

Greenwich Mean Time	$9^h\ 2^m\ 44^s$
Obs. Dist. from *Capella*	40° 20′ 10″
Obs. Dist. from *Aldebaran*	13 38 10
Obs. Alt. of *Capella*	39 35 0
Obs. Alt. of *Aldebaran*	27 45 0
Obs. Alt. of Comet	13 30 0

The comet was so dim that *Sirius* (being in an inconvenient position for holding the sextant) could not be observed.

Index Corr. + 20″; Height of Eye, 18 feet.

Minima of Algol and S Cancri for 1854 (*Mean Time Paris*). By Prof. Argelander.

Minima of Algol.

1854.	h m	1854.	h m
May 3*	12 49	Sept. 29	14 58
6	9 38	Oct. 2	11 46
9	6 27	5	8 35
23	14 31	8	5 23
26	11 20	19	16 38
29	8 9	22	13 27
June 12	16 13	25	10 16
15	13 1	28	7 4
18	9 50	31	3 53
July 5	14 42	Nov. 11	15 8
8	11 30	14	11 57
11	8 19	17	8 46
25	16 22	20	5 35
28	13 10	Dec. 1	16 51
31	9 59	4	13 40
Aug. 3	6 47	7	10 29
17	14 49	10	7 18
20	11 38	13	4 7
23	8 27	21	18 34
26	5 15	24	15 23
Sept. 6	16 29	27	12 12
9	13 18	30	9 1
12	10 6	33	5 50
15	6 55		

"In computing this ephemeris, I assumed, as a starting point, the minimum of 1853, October 23, $10^h\ 7^m\ 37^s$ M.T., Paris, which

* The earlier minima are here omitted.

is founded upon four very consistent minima observed in October and November. Having compared this result with the minimum of 1842, September 19, $22^h\ 45^m\ 22^s$, M.T., Paris, which was deduced from twenty-two observations comprised between the years 1840–7, and which is about 1413 periods earlier, I determined the period to be $2^d\ 20^h\ 48^m\ 53^s{\cdot}46$, with the probable error $\pm\ 0^s{\cdot}2$. A strong proof of a diminution here offers itself, and I have, accordingly, in the computation of the ephemeris, assumed the period to be $2^d\ 20^h\ 48^m\ 52^s$, which cannot be very far removed from the truth. Moreover, since I have employed the equation of light, the calculated minima can deviate only very inconsiderably from the mean of the observations. I say the *mean* of the observations, since it appears to me on the whole that this period also is not very regular, and observers ought not, therefore, to be surprised to meet with individual deviations of from 15^m to 20^m. But I would request observers to dismiss from their minds the exact results of the ephemeris during the observation, so as to guard against the effects of prepossession.

Minima of S Cancri.

1854.	h m	1854.	h m
May 6	1 8	Oct. 23	18 14
15	12 45	Nov. 2	5 51
Aug. 27	20 32	11	17 28
Sept. 6	8 9	21	5 5
15	19 46	30	16 42
25	7 23	Dec. 10	4 19
Oct. 4	19 0	19	16 56
14	6 37	29	3 33

"I have also assigned the minima which occur in the daytime, since the epochs cannot be uncertain to the extent of more than an hour, and, even when the observation of the exact minimum is not practicable, it is always interesting to follow carefully the increase or diminution of brightness. Further, I would recall to mind that the variable *S Cancri* discovered by Mr. Hind is in *Præsepe*, whose position and comparison stars I have given in the *Astronomische Nachrichten*, vol. xxxvi., p. 69. But the position of the star *e* is there erroneously assigned.* It should be

1800. $8^h\ 32^m\ 23^s$ $+19°\ 30'$ 1850. $8^h\ 35^m\ 15^s$ $+19°\ 20'$

e precedes the variable star $7^s{\cdot}20$, and is situate $14'\ 2''$ more to the south."

Captain Shea continues his observations of the Solar Spots, noting their positions on the disk by means of diagrams. He remarks that, during the nineteen days which followed the 19th of February of the present year, not a single spot was visible, although the sun was clearly observed.

* See also *Monthly Notices*, vol. xiii. p. 85.

Observations of the Solar Spots made at the Observatory of Berne during the year 1853. By Prof. Wolf.

1853.	Days of Observation.	Days free of Spots.	Number of new Groups during the Month.
January	21	0	14
February	15	0	10
March	24	1	9
April	21	0	9
May	27	0	8
June	20	0	9
July	27	0	11
August	30	0	14
September	24	1	11
October	24	0	9
November	14	0	6
December	15	0	7
	263	2	117

The following table contains a synopsis of the condition of the sun in respect to the spots during the last five years, one-tenth of the number of spots being added to the number of groups corresponding to each day's observation (in order to take into account the magnitude of the groups):—

	1849.	1850.	1851.	1852.	1853.
January	17	10	9	8	4
February	14	10	10	6	5
March	10	9	7	7	4
April	11	4	6	7	5
May	10	5	7	5	4
June	10	11	6	5	5
July	9	5	3	5	5
August	7	7	6	4	5
September	10	9	7	4	3
October	9	9	6	10	5
November	12	5	6	6	3
December	10	5	7	5	3
Annual Number	129	89	80	72	51

"These numbers indicate the approach of a minimum, which, I feel confident, will occur about the year 1855 or 1856, when, according to my determination of the period of the solar spots, such a minimum might be expected."

Sir Francis Beaufort recently forwarded from the Admiralty a very beautiful chart of the path of Comet II., 1853, as seen at Lord Howe's Island (lat. 31° 31′ 38″ S. and long. 159° 5′ 21″ E.), which was executed by Mr. J. Glen Wilson, H.M.S. Herald. The position of the comet for each day was laid down from angles taken with a sextant between it and the stars *Orion*, *Procyon*, and *Sirius*. The chart exhibits the course of the comet through the stars from the 1st to the 8th of May, when it ceased to be visible.

Elements of Amphitrite. By M. Yvon Villarceau.

Epoch, 1854, March 0·0, M.T. Paris.

Mean Anomaly	114° 36′ 54″·58	
Longitude of the Perihelion	64 50 22·81	Mean Equinox of 1854, March 0·0.
Longitude of Ascending Node	356 20 34·94	
Inclination	6 6 19·69	
Angle of Excentricity	4 34 47·04	
Mean Diurnal Motion	864″·3666	
Semi-major Axis	2·5637307	
Period of Revolution	4^{yrs}·104962	

These elements are founded on the observations of the planet made at the Royal Observatory of Paris during the month of March, including the two original observations of Regent's Park. The method employed is that which is explained in the *Connaissance des Temps* for 1852. Nine normal positions of the planet were made use of. The following is a comparison of the elements with three of these positions:—

	Obs. − Cal.	
	Δ R.A.	Δ Decl.
	s	″
March 1·5	+0·22	+0·9
12·5	+0·13	−1·7
27·5	−0·24	−1·6

In a note upon the comet recently observed, which appears in the *Comptes Rendus* for April 3, 1854, M. Laugier states that M. Alfred de Menciaux, a gentleman residing near Damazan, in the south of France, announced to him that, on the 23d of March, about four o'clock in the *morning*, he perceived a magnificent comet situate due east. The vertical of the comet appeared to be a little more northerly than that of the sun at rising. The tail was almost perpendicular to the horizon, but inclined slightly towards the north. M. Laugier found that a corresponding position of the comet, which afterwards became generally visible on the *evening* of the 29th of March, computed from the elements of its orbit, as deduced by him, afforded a satisfactory account of the observation of M. Menciaux.

[illegible] 3 in latitude.

ERRATUM.

The *date* which Mr. Hippisley assigned to his observation of *Saturn*, of which an account appeared in the *Monthly Notice* for March, was inadvertently omitted in the printing of the paper. Mr. Hippisley stated in his communication that the observation was made on the 12th of February, 1854.

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ROYAL ASTRONOMICAL SOCIETY.

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G. B. AIRY, Esq. President, in the Chair.

George Bishop, Jun. Esq. 39 Portland Place;
Rev. W. Taylor, 73 Oxford Terrace;
Albert Marth, Esq. Mr. Bishop's Observatory, Regent's Park.

On the Orbit of Comet II. 1854. By Edward Cooper, Esq.

"The following being rather a curious agreement of the parabolic hypothesis with the observations, I send it in the hope that it may be acceptable to the Royal Astronomical Society. The observations are mine on March 30, Paris on April 7, and Mr. Graham's and mine conjoined on April 15th.

Late Comet.

G.M.T. 1854.	α h m s	δ ° ′ ″
March 30·369639	1 22 27·34	+19 37 14·3
April 7·324534	3 7 57·46	13 17 3·9
15·371142	4 13 16·36	5 44 2·4

"Corrections for aberration and parallax were made by the aid of Mr. Graham's first set of elements.

Second Approximation by Mr. Graham.

T. March 24·01376, G.M.T.

	° ′ ″
π	213 50 8·9
Ω	315 28 16·1
i	82 30 17·4
log q	9·442544

Retrograde.

"The corrections of the calculated middle place to reduce it to the observed, are

+ o″·1 in longitude, and + o″·3 in latitude.

Observations of Amphitrite, taken with the Meridian Circle at Bonn. By Professor Argelander.

(*Communicated by Albert Marth, Esq.*)

1854.	R.A. h m s	Decl. ° ′ ″	Mag.
March 19	13 5 48·08	−9 44 5·3	9·0
20	4 58·30	41 40·65	9·0
31	12 55 3·84	8 52·6	9
April 2	53 10·85	−9 1 57·6	br. 9
5	50 21·16	−8 51 12·3	9
6	49 24·51	47 32·0	9
9	46 36·95	36 25·9	faint 9
10	45 41·69	32 44·1	9
12	43 52·69	25 14·2	9
13	42 59·01	21 30·8	9
14	42 5·90	17 50·1	f. 9
16	40 21·57	10 28·7	br. 9·0
17	39 30·63	6 52·3	f. 9
18	38 40·73	−8 3 12·0	f. 9
19	12 37 51·31	−7 59 42·2	br. 9·0

On the Final Determination of the Longitude of Cambridge from Observations of Galvanic Signals.

(*Extract of a Letter from Professor Challis.*)

"Soon after the observations were made [in May last year], I communicated to the Astronomer Royal a provisional result, stating, at the same time, that I expected it would subsequently be modified by taking account of the effect of the forms of the transit-pivots. This first result is published in the *Monthly Notices*, Vol. XIII. p. 251. I recently completed the calculation of the corrections to be applied to the transit observations that were taken on May 17 and 18, 1853, for the determination of the true sidereal times of the signals, and have deduced on applying them the subjoined value of the longitude. These corrections were calculated by the method which I have described in the Society's *Memoirs*, Vol. XIX., p. 103, and are the mean results from two independent sets of measures which agreed well with each other. The calculations cost no little time and labour.

East Longitude of the Cambridge Observatory.

Method (A).

	s
May 17, By 146 signals	22·660
18, By 135 signals	22·743
Mean	22·687

Method (B).

	s
May 17, By 146 signals.....	22·601
18, By 135 signals.....	22·782
Mean.....	22·691
Mean of the whole.....	22·689

"Slight errors made in the first calculation being taken into account, the effect of the forms of the pivots is to make the longitude less eastward by $0^s{\cdot}32$. I propose hereafter to adopt $22^s{\cdot}69$ east for the longitude of the Cambridge observatory."

Errata in Weisse's Catalogue and in Lalande.

"The following particulars will show that observers may be put to considerable inconvenience by the imperfection of catalogues:—

"1854, May 8. Weisse xi. 552 was selected for comparison with *Bellona*. No star agreeing with the place of the catalogue is to be found in the heavens, nor in the original zones; but in zone 225 there is a star of the same R.A., and of equal *south* declination, viz. $-12^\circ\ 24'\ 24''{\cdot}1$.

"1854, May 16. No star to be found in the place of Weisse xv. 259, which was selected for comparison with *Thalia*. By reduction from the original zones it appeared that the declination should be $-13^\circ\ 17'\ 8''{\cdot}8$. The star is therefore the same as H.C. 28009.

"1854, May 23. In an attempt to observe *Euterpe* by means of H.C. 13739, no star could be found in the place of the catalogue. J. Challis.

"*Cambridge Observatory, June* 2, 1854."

"The singular phenomenon of a mock moon was visible here on last Thursday evening, 30th March. The evening was perfectly clear and free from all apparent vapour, and as the sun went below the horizon the stars shone forth with the greatest brilliancy. About seven, being on the open fields, and turning to look at the moon, I thought it was larger than usual; and upon stedfastly looking at it, I distinctly saw the light of two moons bearing the following appearance, there being between the two space to the eye of about six inches — the reflected moon giving forth slight motion or shooting effulgences similar to the aurora borealis; whilst above, the whole disk of the moon became quite visible. Many persons saw the same, their attention being called to it, and watched the setting of the moon and its companion in the far west. C. E. Walker.

"*Rectory, Clyst St. Lawrence, Devon.*"

On the Observatory at Stone Vicarage, near Aylesbury.
By the Rev. J. B. Reade, M.A., F.R.S.

As the advowson of the living of Stone has been presented to the Council of the Astronomical Society, it may be desirable, for the purpose of future reference, to place in the Society's archives a short account of the Observatory which has recently been erected on the vicarage lawn.

This Observatory, without any pretension to accurate architectural detail, is an elegant Grecian building, consisting of a transit-room and a tower for the equatoreal. On the site, selected by Admiral Smyth, which commands the passage of *α Columbæ* to the south, and the lower transit of *α Cygni* to the north, 9-inch brick walls, surrounded by an air-drain 2 feet deep by 18 inches in width, were raised on a solid mass of concrete, resting on a natural bed of compact Kimmeridge clay. The transit-room is 11 feet in length, 10 in breadth, and 10 in height, having a flat roof covered with sheet lead. The slits in the north and south walls are 18 inches wide, and reach down below the level of the telescope. The vertical portion of the slits is closed by glazed windows, with shutters outside to exclude the solar rays, and the continuous portion through the roof is fitted with a north and south wooden flap, covered with copper, and opened by rods resting on suitable sockets. The floor is supported on strong joists forming square frames around the transit and clock bases, which are of solid masonry, whilst a free circulation is secured beneath by means of small iron gratings in the walls and air-drain. Three steps to a doorway in the eastern wall lead into a circular room nearly 14 feet in diameter, containing the polar axis, to which the large refractor is attached by an equatoreal mounting; and the wall-plate is capped by an iron channel containing balls of the same metal 4 inches in diameter. The conical dome of well-seasoned Riga fir, cut with the grain, and jointed together with thin laths, was formerly at Bedford, and will always be an interesting relic to those who can appreciate Admiral Smyth's most valuable contributions to astronomical science. It is pleasant now to hear the Admiral describe, under his old dome, where he rode at anchor, the results of his untiring labours; and the resolution with which he passed night after night in the measurement of double stars, never forgetting, to use the words of Shakspeare, his "bright particular star," *γ Virginis*, makes his motto, "Mors aut gloria," almost as applicable to his astronomical as to his naval exploits.

The transit instrument is supported on solid piers of Bath stone cut from a single block, which was purchased many years ago for the Observatory at Hartwell. The object-glass, $4\frac{1}{4}$ inches in diameter and 6 feet in focal length, is an interesting specimen of the skill of Mr. Peter Dollond; and Mr. Barrow, of Oxenden Street, has spared no pains in making the brass-work mounting very strong and of the most approved construction. The transit observations are entered and reduced on the plan adopted at Greenwich.

The object-glass of the equatoreal, 7·3 inches in diameter and 12 feet in focal length, is by Mr. Newman of York, and it well repays the large amount of labour both of calculating and grinding the proper curves. The flint-glass in the rough was purchased by Wm. Tulley for 70*l.*, and Mr. Chance of Birmingham supplied the glass for the crown lens. When the aperture is diminished to about 6 inches, it bears well the *experimentum crucis* of good workmanship, as it shows the concentric rings with equal distinctness when the eye-piece is placed on either side of the focus. Its "*space-penetrating power*" may be judged of by the precision with which the 5th and 6th stars in the trapezium of *Orion* are brought out. The surfaces, however, are not worked up to the highest polish, and there is consequently a felt loss of light when the deeper powers are applied.

The equatoreal mounting made under the direction of Mr. Gravatt by the village carpenter, Mr. George Carter, is generally similar to that adopted by Dr. Lee and Mr. Bishop, but the large 6-feet declination circles, of which one is fixed to the polar axis, and the other to the frame-work of the telescope, are of wood. Rhomboidal bracings of wood attached to the latter circle, and reaching from the object to the eye-end of the telescope, have received the unqualified approbation of many practical astronomers; and their construction is well shown in the beautiful model of the Observatory which accompanies this paper, and which has been made by Mr. Charles Carter, the son of the village carpenter before-mentioned.

The declination axis with the large iron cradle, in which the telescope lies, as well as the pivots and iron-work at either end of the axis, is the work of Mr. Donkin. The box of the declination axis is *square*, and not round, as is usually the case, and, in order to command a most important adjustment, it is inserted into a nearly square hole in the polar axis (the longer side being in the direction of the length of the axis), so that by means of a couple of wedges driven into this hole towards each end of the axis, the parallelism of the axis of the telescope and of the polar axis is perfectly attained. In this position the telescope points to the true pole during the whole revolution of the hour-circle, and the adjustment once made, can never be disarranged except by violent strain.

The telescope is furnished with a position wire micrometer by Tulley, and the usual range of astronomical powers; and an hour-circle, to be driven by clockwork, will be added by Mr. Whitworth of Manchester.

A complete set of meteorological instruments has been obtained under the direction of Mr. Glaisher, and monthly observations are regularly forwarded to the Registrar-General.*

* With respect to the achromatism of my solid eye-piece, which obtained a prize-medal at the Exhibition, Mr. Dawes says, in a note just received: — "I have only time to say that, compared with a Coddington lens, which I have, and which, curiously enough, happens to be of precisely the same focal length,

The *latitude* of the Observatory determined by a Troughton's reflecting circle is 51° 47′ 57″·0 north, and the *longitude*, from simultaneous observations of moon-culminating stars at Oxford, Cambridge, and Hartwell, is $0^h\ 3^m\ 29^s{\cdot}09$ west.

The following is a general statement of the cost of the building and of the instruments, Dr. Lee having most handsomely presented 15,000 bricks, as well as the piers and dome :—

	£	s.	d.
Bricklayer and stonemason	51	4	6
Carpenter	75	6	1
Mr. Gurney, for wood	47	10	6
Plasterer	25	14	0
Plumber and glazier	28	12	0
Iron-work	16	1	2
	£244	8	3

	£	s.	d.
Timber and work for polar axis	24	19	9
Declination axis	19	0	0
12-Foot refractor	100	0	0
Transit instrument	80	0	0
Meteorological instruments	20	0	0
	£243	19	9

The whole expense, consequently, amounts to £488 8*s*.

Stone, May 9, 1854.

Occultation of Mars observed at Highbury. By T. W. Burr, Esq.

"1854, March 12th. This occultation was observed with my 4-foot equatoreal with power 173.

	h	m	s	
The Immersion began at	14	33	19	Sidereal Time.
The Immersion complete at...	14	34	17	
The Emersion complete at ...	14	58	38	

"The observation of the first contact is a little uncertain, probably 1^s too late, it being difficult to detect the earliest impression made on the planet's limb. The complete eclipse was very accurately noted, as also the complete emersion. The reappearance of the first limb was lost while changing an eye-piece. The time was

and giving ∴ both of them a power of 358 on a telescope of 102½-inch focus, your solid achromatic has the advantage of being almost entirely free from colour throughout the field, which, though small, is quite equal to that of the Coddington, when the eye-hole is removed from your lens. The eye-hole is too small, not admitting oblique pencils, which are very pure and good; though it is large enough to admit all the rays which come from the object-glass nearly direct." "The smallness of the field (for deep powers) may prevent this from being in general a desirable form for a micrometer eye-piece, though it may prove, when carefully constructed, an agreeable and efficient one for delicate scrutinies of small objects."

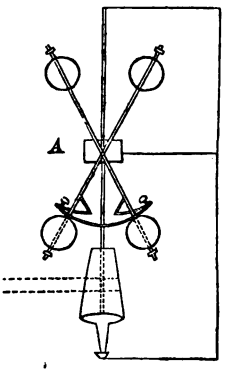
corrected by a transit of α^2 *Libræ*, taken during the occultation. No projection of *Mars* on the moon's disk was seen or other anomalous appearance. The definition was very fine, the configuration of the planet's surface being traceable to the last moment of eclipse by the moon's limb without any diminution of sharpness which would have been the case with an extensive lunar atmosphere."

Notes on Experiments relative to Lunar Photography and the Construction of Reflecting Specula. By the Earl of Rosse.

(*Extract of a Letter from the Earl of Rosse to the Astronomer Royal.*)

"As you mentioned to me in one of your letters that the Astronomical Society would be glad to hear from time to time what we are doing, there are two or three little matters I have been recently engaged in which may, perhaps, interest them.

"First, as to lunar photography. I have constructed a smooth motion-clock to carry the plate of glass, and its performance is satisfactory. The regulator is thus made: there are two levers with balls on the extremities, which exactly balance in *every position;* they are acted upon by two springs with screw adjustment, and on the expansion of the balls the regulating friction takes place at the ring A.* The object was to obtain a regulator independent of position. The direction of the motion of the glass plate is regulated by an adjustable slide, and we set the slide by trial, not by a table computed for the purpose. To set the slide, an eye-piece, with lines truly parallel to the slide, is inserted. By such means a pretty picture of the moon can be obtained, but at present I believe there is no known photographic process which is sufficiently sensitive to give details *in the least degree* approaching to the way in which they they are brought out by the eye. The application of such a smooth motion-clock to instruments *not equatoreally* mounted, may, perhaps, be important, as it affords great facilities for the use of the micrometer. With our 3-foot telescope, I have no doubt, excellent micrometric measures might be obtained; and with a somewhat enlarged small speculum, there would be ample time without hurry. For all objects but the moon, a table might be constructed with little trouble for setting the slide, which would save time.



"You recollect, no doubt, how greatly superior silver would be to speculum metal, if it could be as well and as easily polished as speculum metal. At the Ipswich meeting of the British Association

* The crossed rods pass in the inside of a ring and rub the inner edges of the ring.

I described a process which had been, to a certain extent, successful. It is difficult, however, and uncertain; and as a silver surface, is very perishable; it would scarcely be worth while to employ it, except under special circumstances.

"Another method which I have very recently tried is perfectly easy, and promises well. A plate of glass is coated with silver by precipitation from saccharate of silver. The silver film is then varnished with tincture of shell-lac, and when dry the temperature of the glass is gradually raised to the fusing point of shell-lac. Pieces of shell-lac are then laid upon it, and over them a piece of thick glass. A slight weight presses out the superfluous shell-lac, and the whole having gradually cooled, the silver film adheres permanently to the shell-lac, the glass upon which it had been originally precipitated being easily removed without injuring it. We have thus a silver surface apparently as true as the glass upon which it had been precipitated, and with a beautiful polish. The experiment is imperfect so far as this, that as yet merely common plate-glass has been tried, and not a true glass surface; and as I am about to set out for London, I shall have no opportunity for some time of completing these experiments.

"With the view of applying Mr. Lassell's levers to one of our 6-foot specula, should there be a reasonable prospect of improving its performance in that way, I have tried some experiments as to the practicability of drilling speculum metal. I find it can be drilled by a tubular drill of soft iron and emery, the core being from time to time removed by a pointed chisel and a very light hammer, by which it can be safely broken up gradually. A drill with diamonds set in a groove cuts it well also; and even a drill of perfectly hard steel, revolving slowly, cuts it well; so that there can be no serious difficulty in making the necessary perforations.

"*Castle, Parsonstown, April 20th*, 1854."

The Astronomer Royal having resigned the chair to Mr. Sheepshanks, proceeded to give a full explanation to the meeting of the various experiments alluded to in the foregoing communication from Lord Rosse, his remarks being rendered readily intelligible by means of models, which he had caused to be constructed for the purpose of illustration, and which had been obligingly forwarded by him to the Apartments of the Society. He pointed out the advantage which the system of supports for resisting edgewise pressure, now proposed by Lord Rosse, would have over that of Mr. Lassell, of which it was a modification, namely, that in the case of a reflector being mounted equatoreally, it would prevent the possibility of undue pressure against the *side* of the supporting-ring during the period when the telescope was being pointed to a celestial object; whereas in Mr. Lassell's system the supports come into their proper operation only after the position of the speculum has been rectified by rotating the tube in its cradle: this is a point of great importance in large specula, as it is found that they do not immediately recover their normal figure after distortion by pressure. At the same time, he

took occasion to repeat that he still thought it preferable to avoid the rotation of the telescope tube in its cradle altogether, which the altazimuth mounting he had proposed would effect, and yet still provide for an equatoreal movement in the telescope, although he was aware that Mr. De la Rue was of opinion that the difficulties of mounting very large telescopes on an equatoreal stand might be overcome. The Astronomer Royal also pointed out, that if such a mounting were adopted, it would be desirable to so construct the system of supports for resisting the pressure perpendicular to the surface of the mirror, as to admit of the fulcra of the levers for resisting edgewise pressure being carried by them, in order that the ends of the levers might support the pressures in all directions equably, without constraining the mirror in any way, and without impeding its change of position by any sensible friction.

The Mediterranean: a Memoir Physical, Historical, and Nautical. By Rear-Admiral William Henry Smyth, K.S.F., D.C.L. &c. &c. 519 pp., 8vo. London: John W. Parker and Son. 1854.

In this volume Admiral Smyth has presented the results of many years of personal observation and research in the Mediterranean, bringing to the aid of his labours a vast amount of valuable scientific, literary, and antiquarian lore. As a contribution to Physical Geography, the merits of this work are obvious at first sight, and have not failed to be duly appreciated by the most competent authorities on the subject; but it is no less calculated to advance the objects of Nautical Astronomy, considered in its most comprehensive signification as a branch of mathematical and physical inquiry. The work is divided into five parts. Part I. contains a chorographical view of the shores of the Mediterranean, with especial reference to their produce and commerce. This may be considered as introductory to the scientific part of the work. Part II. treats of the currents, tides, and waters of the Mediterranean Sea. Part III. is on the winds, weather, and atmospherical phenomena of the regions of the Mediterranean. In Part IV. the author gives an interesting account of the early surveys of the Mediterranean, and concludes with an exposition of his own labours, having for their object the improvement of the existing state of knowledge on the subject. The valuable character of Admiral Smyth's Marine Surveys, — which extended over the interval comprised between the years 1810 and 1824, — was soon recognised by the Admiralty, who caused charts of them to be engraved and published, which have been long in general use. In the fifth and concluding part of his work, the author gives the determinations of the Maritime Positions upon which his charts were based. It contains

* In connexion with these remarks, the reader may consult with advantage the substance of a lecture delivered by the Astronomer Royal on the large reflecting telescopes of the Earl of Rosse and Mr. Lassell, which is published in vol. ix. of the *Monthly Notices* (p. 110, *et seq.*).

the longitudes and latitudes of about 1500 points distributed on the coasts of Spain, France, Italy, &c. &c. The remarks accompanying each determination are expressed by means of symbols, which are the same as those used by Lieut. Raper in his well-known treatise on the Practice of Navigation. The Admiral suggests the expediency of introducing a few additional symbols, but he assigns to his friend, Lieut. Raper, the execution of this task on the occasion of publishing the next edition of his standard work.

Die Wunder des Himmels oder gemeinfassliche Darstellung des Weltsystems. Von J. J. von Littrow. Vierte Auflage. Nach den neuesten Fortschritten der Wissenschaft bearbeitet von Karl v. Littrow, Director der Sternwarte zu Wien, &c. &c. 8vo. (pp. 833), Stuttgardt, 1854.

J. J. von Littrow's Atlas des gestirnten Himmels fur Freunde der Astronomie. Zweite vielfach verbesserte und vermehrte Auflage, herausgegeben von Karl von Littrow. Stuttgardt, 1854.

The work on popular astronomy by the late Professor Littrow has long been much esteemed in Germany, as is proved by the appearance of four successive editions. The present edition has been adapted to the existing state of astronomical science. The work is divided into four parts. The first part treats of theoretical astronomy; the second part is upon descriptive astronomy and the topography of the heavens; the third part is devoted to physical astronomy; and the fourth is upon the description and use of astronomical instruments. The Atlas above entitled forms a suitable accompaniment to the work.

Report of the Astronomer Royal to the Board of Visitors, read at the Annual Visitation of the Royal Observatory, Greenwich, 1854, June 3.

The present Report to the Board of Visitors may be understood as recording the general condition of the Observatory on 1854, May 26, and the transactions from 1853, May 22, to that time.

Grounds and Buildings.—I alluded in the last Report to the necessity for a new fire-proof room. The want of this became so evident that in the last autumn I submitted to the Lords Commissioners of the Admiralty a proposal for constructing one in the present year; and in consequence a sum of 550*l.*, applicable to that purpose, has been granted on the Navy Estimates. The plan has been arranged in concert with Lieut.-Colonel Greene, R.E., civil architect of the Admiralty. * * * * †

Astronomical Instruments.—The transit-circle has not received or required any alteration. Its efficiency is perfect.

The new mounting of the quicksilver-trough, to which I alluded

† Several entire paragraphs are here omitted: the same is to be understood of those other parts where asterisks occur.

in the last Report, is in use, and is found advantageous, as I expected. It has required a new crane cistern for the store of quicksilver. Troughs both of iron and copper are adapted to the mounting, and sometimes one and sometimes the other is employed, according as the observer prefers to submit to the inconvenience of tremor with the iron trough, or loss of light with that of copper.

The transit-circle for the Cape Observatory, at the last advices, had been safely received at the Cape, but was not mounted.

The reflex zenith-tube is in general good order, and is, generally speaking, perfectly efficient. Still I think that its efficiency will be improved, as regards both the number and the excellence of the observations, by transferring it to a site where it will be less subject to tremor.

The altazimuth is in the same state as in the last years, and is completely efficient. Still I cannot deny that, with the experience that I have gained in the construction of the transit-circle, I could now construct a better altazimuth—better, however, rather in convenience than in accuracy. In the particular point which I mentioned in my last Report (the counterpoise friction-wheels of the horizontal axis), I still contemplate a change.

The barrel apparatus for the American method of transits has been practically brought into use; not, however, without a succession of difficulties, arising from causes sometimes very hard to discover. When the instrument was approaching to a serviceable state, there still remained an imperfection in the ill-defined form of the punctures on the paper. At this juncture, Lieutenant Maury, U.S.N., paid me a brief visit, and in the course of inspection of the instruments he alluded to this very defect, and to the method which had been used in America for its remedy. Although my apparatus did not admit of the same application, yet, possessed of the principle, I had no difficulty in embodying it in a form adapted to my wants; the prickers were mounted on springs, and now the punctures are perfectly round. The paper on which the punctures are to be made is folded in a wet state, upon a brass cylinder covered with a single thickness of tailor's woollen cloth, and has its edges united by glue.

The punctures, it will be remembered, are produced by two systems of prickers, which have nothing in common except that they are carried by the same travelling frame, which moves slowly in the direction of the barrel-axis while the barrel revolves beneath it. These require separate notice.

One pricker is driven by a galvanic magnet whose galvanic circuit is completed at every second of sidereal time. It was at first intended by me that the completion of the circuit should be effected by the same smooth-motion clock (regulated by a conical pendulum) which drives the barrel. I found, however, that I could not ensure such a constancy in the radial arc of the pendulum as would make its rate sufficiently uniform to entitle it to be considered as the fundamental clock; and, moreover, there was a little difficulty in referring its indications to those of the transit-clock

(which must be used in some cases). I, therefore, carried wires from the pricker-magnet to the transit-clock, connected there with springs whose contact is made at every second by the transit-clock. At first the contact was made by the touch of a pin fixed in the pendulum-rod; and this construction for a time answered well. But it so happens that, in our transit-clock, the pendulum is carried by one frame, and the point of attachment of the galvanic springs by a different frame: it was impossible to maintain these in steady adjustment; and the rate of the clock was sensibly disturbed. I have now adopted the following construction, which promises to succeed better. A wheel of 60 teeth is fixed on the escape-wheel axis, and the teeth of this wheel in succession make momentary contacts of the galvanic springs. The position of the springs is so adjusted that the effort of the wheel-tooth upon them occurs only when one escape-tooth has passed the sloping surface of the pallet, and the other escape-tooth is dropping upon its bearing; and thus the resistance of the springs does in no way affect the legitimate action of the train upon the pendulum.

The other pricker is driven by a galvanic magnet, whose circuit is completed by an arbitrary touch made by an observer's finger upon a contact-piece. Of contact-pieces there are three. One is upon the eye-end of the transit-circle: it effects the contact of two brass rings which (by means of wires passing in the interior of the tubes) are connected with two other brass rings surrounding the axis and touched respectively by two springs on the pier leading to the galvanic wires. The other two contact-pieces are upon the rotating base-plate of the altazimuth (one to be used with vertical face to the right, the other with vertical face to the left); the parts which they bring together carry springs which touch two large horizontal rings on the fixed base; and these rings are connected with branches of the same pair of wires which communicate with the altazimuth. Thus altazimuth observations are referred absolutely to the same time-record as transit-circle observations.

It is necessary to mark upon the revolving barrel the beginnings of some minutes and the numeration of some hours and minutes. This is done by arbitrary punctures given by the observer's touch, upon a simple system which scarcely merits detailed description.

In order to guide the eye through the multitude of dots upon the sheet, lines of ink are traced by means of a glass pen, which is attached to the same frame as that by which the prickers are carried.

Wires have been inserted in the wire plates, both of the transit-circle and of the altazimuth, at intervals adapted to the rapid observation by touch. The wires of the transit-circle and the vertical wires of the altazimuth are adapted to intervals of about 42″ and 48″ of arc; the intervals of the horizontal wires of the altazimuth do not exceed 24″ of arc. They are probably the smallest intervals that have ever been used. The old systems of wires are not disturbed, nor rendered confused; so that, with the transit-circle, either 7 wires may be observed by ear, or 9 by touch; and

with the altazimuth (in either dimension) either 6 by ear or 6 by touch.

I have only to add that this apparatus is now generally efficient. It is troublesome in use, consuming much time in the galvanic preparations, the preparation of the paper, and the translation of the puncture-indications into figures. But among the observers who use it there is but one opinion on its astronomical merits; that, in freedom from personal equation and in general accuracy, it is very far superior to the observation by eye and ear.

The galvanic apparatus for giving time-signals, for dropping the time-ball, and for maintaining the movement of sympathetic clocks, is in the same state as at the last Report, with this addition only,—that a separate battery, galvanic needle, and pair of contact springs, are appropriated to the wire on which the current is sent to drop the time-ball at Deal at 1^h (the contact being completed by the same movement of a galvanic magnet which drops our own ball), and that the fall of our ball-tumbler then effects such a change in the connexions of the wires, that the galvanic needle is in a state fit to receive the return-signal given by the Deal ball when it reaches the end of its fall.

The hour-circle microscopes of the north, or Shuckburgh's equatoreal are in Mr. Simms's hands for alteration. In other respects, the equatoreals, micrometers, &c., are in their usual good state. Preparations have been made for attaching a photographic apparatus to the east, or Sheepshanks' equatoreal.

The old instruments of the Observatory have been preserved almost untouched since the last meeting of the Visitors.

Observations.—The long-established plan of meridional observations is preserved unaltered in its essential points. The standard stars of the extended list, as well as those of the *Nautical Almanac* list, are observed (the latter twenty times in three years, if possible), and all the bodies of the solar system at every opportunity (the moon only being observed on Sundays). The whole number of observations from 1853, May 22, to 1854, May 26, is nearly as follows (an observation of two limbs, or a duplicate transit by eye and by touch, being counted as two):—In the transit department: transits, 4802; observations of collimators, 317; observations of transit-wires by reflexion, 317; observations of one collimator by the other, 53. In the meridian-circle department: observations of all kinds, 4590.

The intervals of transit-wires have been determined by collimator-observations when it appeared necessary.

The ordinary routine of transit-observations is the same as last year. Of clock stars and planets it is unnecessary to speak: the adjustment-observations are,—weekly comparisons of collimators, daily observations of collimators with the transit-telescope and of the wires by reflexion, observations of circumpolar stars when possible. It has been necessary to add to these, comparisons of the transits by ear and by touch, as the ear-observation must be used still for the very slowly moving stars.

For the circle adjustments, there are weekly observations of runs of microscope-micrometers, daily observations of the wire by reflexion, and observations of stars by reflexion when practicable.

With the reflex zenith tube, observations of γ *Draconis* have been obtained on 39 days. The principle of using quicksilver in a copper pan has not been adopted here.

The observations with the altazimuth have been conducted as in the last two years. The number of days of complete observations of the moon is 209, or 16·7 per lunation, against 117 with the transit circle, or 9·4 per lunation. Of the altazimuth observations, 0 are when the moon passed the meridian between 0^h and 1^h, 3 between 1^h and 2^h, 9 between 2^h and 3^h, 5 between 21^h and 22^h, 0 between 22^h and 23^h, 1 between 23^h and 24^h. There is only one corresponding observation on the meridian, namely, between 2^h and 3^h. The whole number of separate observations of moon and stars with the altazimuth is 976; and the whole number of separate observations of its collimator, 712.

The equatoreals have been used for observing the third comet of 1853, and the great comet of 1854. The former observations do not, perhaps, possess any particular value. No observations have been made on the solar spots.

The double-image micrometer has been used for measures of γ *Virginis* (two evenings), and of *Saturn's* rings (six evenings).

Reduction of Observations.—Although our reductions are in a perfectly healthy state, quite complete in everything that demands judgment and recollection of the circumstances under which the observations were made, and now requiring only masses of mechanical calculations, yet they are not so forward as I could wish. They are, however, almost more forward than I could expect; for very much time has been consumed in general galvanic arrangements, in the arrangements for the galvanic drop of a time-ball at Deal, in the reducing to practical efficiency the American method for transits, and in the operations for the longitude of Brussels; and very much time also in the reading of the great number of proof-sheets lately received. The maintenance of our computations, under these circumstances, in a forward state, is due almost entirely to the skilful and orderly arrangements and the unwearied personal attention of Mr. Main.

For transits, the clock times of true transits are formed to May 13; the apparent right ascensions from observation to May 12; the star corrections are computed to May 20, and are applied to March 5. For the circle observations the concluded circle readings are completed to May 20; the apparent zenith distances to May 6; and the north polar distances from observation to April 1. The star corrections are computed to April 1, but are not yet applied.

The observations with the reflex zenith tube are reduced to May 19.

With the altazimuth, the zenith distances are completely reduced to April 20, and the azimuths to Feb. 22; the tabular computations of zenith distance and azimuth are complete to

March 17; and the errors of zenith distance and azimuth are converted into errors of R.A. and N.P.D. to the end of 1853. The results are very satisfactory.

The observations with the double-image micrometer are fully reduced.

For 1853, the means of the star-places in ledger are completely or very nearly finished, but the star-catalogue is not commenced. No effective work is done to the solar, lunar, and planetary reductions, beyond the formation of the separate errors in R.A. and N.P.D.

As soon as the observations of 1853 shall be sufficiently reduced, I propose to prepare a new star-catalogue, based on the six years' observations from 1848 to 1853.

Printing of Astronomical Observations.—I stated in my last Report that the printing of the Observations for 1852 was scarcely commenced at the time of the last meeting of the visitors. For a long time the printing went on so slowly that I almost despaired of ever again seeing the Observations in a creditable state. After a most harassing correspondence, the printers were at length persuaded to move more actively, and for some time the printing advanced at the rate of five sheets per week. This, of course, has contributed greatly to bring the press-work into proper condition; but the volume is still very much behind its usual time of publication.

The astronomical part is (with the exception of a small part of the introduction) finished. The magnetic results are printed as far as the month of May.

Two appendixes are printed, which I trust the visitors will regard with interest. One is, an elaborate description of the transit-circle, illustrated with sixteen plates. The proofs of the plates have been received, but they are not finally passed for working off. The other is a statement of the regulations, public and private, of the Royal Observatory. These were drawn up in consequence of an application of a foreign State to the British Government; and the Lords Commissioners of the Admiralty ultimately decided that the paper should be printed for public reference.

A large quantity of the manuscripts of observations and reductions for 1853 is prepared for the printer, and has recently been forwarded to him for printing. * * * *

Printing of Magnetical and Meteorological Observations. — The magnetical indications are printed to the end of May 1852. No other part of this section is printed for 1852: the whole of the manuscript is in the printer's hands.

Under this head I may appropriately record the efforts that we have made for multiplying copies of our photographic registers. The subject had long been in our contemplation, and some apparatus, suggested by a gentleman to whom we are very greatly indebted for the efficiency of our photographic operations, had been long since prepared. For some time, perhaps, I trusted too much to the assistance which might be obtained from [illegible]

extraneous to our own establishment. In the last winter, however, Mr. Glaisher took up the matter with his usual vigour. It was soon found that by the agency of the sun-light upon the back of an original photograph, whose face was pressed closely by means of a glass plate upon proper photographic paper below, there would be no difficulty in preparing negative and inverted secondaries, and, from them, positive and erect tertiaries. The process is so simple, and admits of repetition to such an extent, that it may be considered infallible; and all that was requisite was, that the primaries should be good. By diminishing the quantity of iodide of potassium in the preparation of the paper; by slightly touching the dark curves, as soon as they are brought out, with nitrate of silver; and by diminishing the quantity of hypo-sulphite of soda; very pure grounds with black lines are obtained. The sheets are fitted up with their apparatus of time-scales, base-lines, &c., and are then waxed: secondaries and tertiaries are taken from them; and the last, if the operation is properly conducted, are in every respect as good for ulterior uses as the originals. Beyond the trouble to ourselves (accompanied with some small expense) of making secondaries, and the expense of making tertiaries through the instrumentality of some professional person, I anticipate no difficulty in multiplying copies to any extent which the visitors may desire.

Chronometers, Communication of Time, and Operations for Longitude.—The number of chronometers now on hand is about sixty, of which sixteen are makers' chronometers on trial. The chronometers are rated daily or weekly; the trial-chronometers are rated in heat and in cold; the abstracts of rates are formed; and the repairs are superintended, as in former years.

The normal-clock, with its small adjusting apparatus (described in last year's Report), has been in constant use, and has been found exceedingly convenient. It drops the Greenwich ball and the Strand ball, it sends daily signals along several railways, and it maintains in sympathetic movement several clocks by galvanic currents. Among other clocks thus moved, one is in the chronometer-room, one is at the entrance gate, and one is at the South-Eastern Railway Offices, London Bridge.

With the sanction of the Commissioners of Greenwich Hospital, it has lately been arranged by the Rev. George Fisher and myself that two wires shall be swung across the lower part of Greenwich Park, from the top of the Octagon Room of the Royal Observatory to the Observatory of the Hospital Schools, for carrying the galvanic currents which will maintain the movement of a sympathetic clock at the School Observatory.

I alluded, in my last Report, to the erection of a time-signal ball at Deal, to be dropped every day by a galvanic current from the Royal Observatory. The ball has now been erected by Messrs. Maudslays and Field, and is an admirable specimen of the workmanship of those celebrated engineers. The galvanic connexion with the Royal Observatory (through the telegraph wires of the South-Eastern Railway) is perfect. The automatic changes of

wire-communications are so arranged that, when the ball at Deal has dropped to its lowest point, it sends a signal to Greenwich to acquaint me, not with the time of the beginning of its fall (which cannot be in error), but with the fact that it has really fallen. The ball has several times been dropped experimentally with perfect success; and some small official and subsidiary arrangements alone are wanting for bringing it into constant use. I can scarcely convey to the Visitors how much I am indebted to the South-Eastern Railway Company and the Electric Telegraph Company, and to their principal telegraph officers, Charles V. Walker, Esq. and Latimer Clark, Esq., for the liberality and even the zeal with which they have assisted me in every step of these preparations. Without the cordial aid of Mr. Walker, in particular, it would have been impossible to complete the work. The best line of wires on the railway has been devoted to this purpose, and the shifting connexions have been modified to diminish the resistance and remove the chances of disturbance as much as possible.

No step has yet been taken for the galvanic determination of the longitude of Oxford Observatory, although I believe that the preparations within that building are now complete.

The determination of the difference of longitude with the Paris Observatory had long been contemplated as one of the important uses of our galvanic connexions. In the last summer and autumn, I recommenced correspondence with Messrs. Arago and Laugier for this object; and one of my letters to M. Arago was addressed to him on the very day of his death. Deeply do I grieve that this operation was not finished in time for him to know the results: not a day, however, had been lost in pressing it forward. It was obviously impossible then to proceed with it; and I considered myself at liberty to take measures in concert with M. Quetelet for our difference of longitude with Brussels. The Submarine and European Telegraph Company, through an unusually protracted operation, gave their warmest assistance. A most able assistant from the Brussels Observatory, M. Bouvy, came to Greenwich, and Mr. Dunkin went to Brussels: when the operation was half finished, the observers returned to their original posts. The signals were continued every night (an hour each night) until three nights' signals had been obtained, accompanied with unexceptionable transits, in each part of the operation; and these alone were retained as available for longitude. Thus, about 3000 effective signals were made, but only 1000 of these were admissible for the fundamental objects of the operation. The result, I need scarcely remark, claims a degree of accuracy to which no preceding determination of longitude could ever pretend. I apprehend that the probable error in the difference of time corresponds to not more than one or two yards upon the earth's surface.

One of the earliest steps taken by M. Le Verrier, after his appointment to the charge of the Observatory of Paris, had reference to the determination of our longitude-difference. All important preliminaries are now arranged, and I trust to be able to report

orally to the Visitors the commencement, or probably the conclusion, of this long-desired operation.

The perfection of our galvanic connexion with Edinburgh having been fully established by the operations of last year, I had proposed to use it for the determination of the longitude of a more distant point. For checking the azimuthal accuracy of our great National Survey, as well as for giving (by interpolation) the error of longitude of every point of our coasts, I had intended to determine with the utmost accuracy the longitude of Lerwick, in Shetland. I proposed to establish a galvanic connexion with a comparing-observatory on the Granton Pier, and to transmit the time from that point to a selected station in the neighbourhood of Lerwick, by a large number of chronometers carried in the steamboat which plies between Granton and Lerwick. With the sanction of the Admiralty, a grant of money for defraying the expenses was inserted in the Navy Estimates; and other preparations were made. Unfortunately, the demand for chronometers caused by our large naval armament has been so considerable that I cannot reckon on having at my disposal a sufficient number to carry on this operation successfully; and I have, therefore, unwillingly deferred it to a more peaceful time. * * * *

General Remarks.—The past year has, on the whole, been felt as a laborious one. This has arisen from a cumulation of several perfectly distinct causes. The order of our printing has been disturbed, and this has produced great disarrangement of all our ordinary daily work. The establishment of our galvanic system, and its application to American transits, to public time-signals, and more especially to the longitude-determination, has caused to the establishment in general, and to myself in particular, a great consumption of time. The preparation of the Observatory Regulations, and of the Description of the Transit-Circle, and the closing of the business of the Standard Commission, have required a great amount of writing which could be intrusted to no one but myself.

I may confidently hope that in the next following years several of these causes will not be in action. Still I am impressed with the feeling that the strength of our establishment is now loaded to the utmost that it can bear. A brief review of the progress of the science of astronomy and of the arts related to it will show that this must be expected. The number of known planets has been largely increased: and I cannot think that in this National Observatory the neglect of any one of the bodies of the solar system is permissible. The American method of transits adds to our labours; but it appears likely to contribute to accuracy, and it will give facilities for the record of the observations made at other observatories, upon our registering-barrels; and, if these advantages are established by experience, the method must be maintained. The public dissemination of accurate time brings some trouble; but it is a utilitarian application of the powers of the Observatory so important that it must be continued. The galvanic determination of difference of longitude brings with it a mass of

work in negotiations, in preparations, and in calculations; but it produces results of such unimpeachable excellence, and of such value to astronomy and geodesy, that it must in any wise be preserved as part of our system. Time is consumed in experiments for the improvement of our photographic process, and in measures for the multiplication of copies; but these are worthy objects of attention, which it would be wrong to neglect. All these are additions to the labours of the Observatory as they existed a few years ago, unbalanced by any corresponding subtraction.

This increase of labour has been met (or partially met), not by an increase of the number of fixed assistants, but by an increase of the number of supernumerary computers. And this is, in my opinion, the cheapest and the most efficient way of supplying our want. The position of the permanent assistants is in some degree altered by this organisation. Formerly they were simple observers and simple computers: now, without losing the responsibility of personal work, each of them has a subordinate computer, whose work is placed almost entirely under his control. The admirable conduct of all the assistants and computers on the establishment justifies me in saying, that the characters of all parties rise under this arrangement.

I think it quite probable that I may desire from the Government an extension of the discretionary powers under which I have engaged these aids. But I do not at present foresee the necessity of any further increase of our fixed personal staff, or of any other source of important expense. G. B. Airy.

Royal Observatory, Greenwich, 1854, May 26.

Lieut.-Colonel De Rottenburg has forwarded from Kingston, Upper Canada, a short communication respecting Comet II. 1854. On the 4th of April he observed it with a telescope by Dollond of $3\frac{1}{4}$ feet focal length. It appeared as bright as a star of the third magnitude. The nucleus was round and well defined. The tail appeared to be about 5° in length, and to have the form of an extremely elongated ellipse. A star of the fourth or fifth magnitude was very distinctly seen through the tail towards its upper and eastern portion.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIV. *June* 9, 1854. No. 8.

G. B. AIRY, Esq. President, in the Chair.

James Samuel, Esq., 26 Great George Street, Westminster;
Lieut. Andrew Noble, Royal Artillery;
Lieut. Wm. Hett Noble, Royal Engineers; and
James Burman, Esq., Castle Rushin, Isle of Man,

were balloted for and duly elected Fellows of the Society.

Dr. Luther, of Bilk,

was balloted for and duly elected an Associate of the Society.

The Astronomer Royal has the pleasure to communicate to British astronomers the following letter, which he has received from the Secretary of the Post-Office. The Astronomer Royal wishes at the same time to direct the notice of astronomers to the liberality and obliging attention of the Secretary of the Post-Office, who, without further application from himself, has spontaneously adverted to the practices in the introduction of the *Astronomische Nachrichten* to this country.*

"*General Post-Office, 14th June*, 1854.

"Sir,—Referring to the recent correspondence upon the subject of the charges which had been made in this country upon certain copies of the *Astronomische Nachrichten*, transmitted through the Post from Hamburgh, I beg leave to inform you that the Director of the Posts at Hamburgh has forwarded to this office some copies of the publication in question, with their covers, from which it appears, that through some inadvertency these copies, and possibly other copies, have been charged with postage in this office, although it is well known to the officers, through whose hands these publications usually pass, that they are entitled to be delivered without charge.

* The *Astronomische Nachrichten* is now regularly forwarded to this country free of postage, and the blank occasioned by the non-transmission of several numbers is being gradually filled up.—EDITOR.

"I very much regret these mistakes which have occasioned so much annoyance, and I have given stringent instructions to the various officers concerned to prevent by all means any future errors of the same kind.

"I have also communicated with the Director of the Posts at Hamburgh to the same effect.—I am, Sir, your obedient, humble Servant,

"J. TILLEY,
"Assistant-Secretary.

"*Professor Airy,*
"*Royal Observatory, Greenwich.*"

Discovery of a New Comet (Comet III. 1854.)

(Letter from Professor Argelander to the Astronomer Royal.)

"On the 4th of June Herr Klinkerfues, in Göttingen, discovered a new comet. As I have just learned that the discoverer has not himself made this appearance known to you, I do myself the honour of informing you; and I add at the same time the positions obtained by the discoverer himself, as well as the only ones which the weather has until now allowed us to make. The comet is very bright, and could be seen very well in the finder the day before yesterday, very near to the horizon, notwithstanding the light of the full moon. It has a remarkable thickening near the middle, but neither a defined nucleus nor a tail.

"The places are derived only from rough calculation:—

Date.			R.A.		Decl.	
	h m s		h m s		° ′ ″	
June 4	14 6 0	Göttingen	2 1 15		+32 15 —	
5	13 37 27	—	2 5 23·25	•	+33 40 18·6	
	13 45 19	—	2 5 24·22		+33 40 43·9	
			° ′		° ′ ″	
11	12 2 48·0	Bonn	40 47·79		+43 56 44·7	
	12 33 30·4	—	40 58·07		+43 59 13·0	

Bonn, June 13, 1854.

The following elements of this comet have been calculated from the Göttingen Observations of June 5, and the Berlin Observations of June 9 and 17, by M. Klinkerfues:—

T	June 22·12174	
π	° ′ ″ 272 58 5·5	Equinox, 1854·0
Ω	347 48 44·9	
i	71 8 21·0	
Log *q*	9·811244	

Motion retrograde.

These elements have some resemblance to those of the comets of 961 and 1558.

Observations of Comet III. 1854, *at Mr. Bishop's Observatory, Regent's Park.*

June 28, at $11^h\ 36^m\ 49^s$ G.M.T.; R.A. 110° 42′ 26″; Decl. + 59° 42′ 58″.

The following calculated places of the comet have been furnished by Mr. Hind:—

0h Gr. M.T.	α	δ	Δ	r
	° ′	° ′		
July 16	150 52	+39 0	1·406	0·818
26	157 36	29 31	1·668	0·951
Aug. 5	161 39	22 22	1·922	1·097
15	164 17	+16 8	2·330	1·247

An Account of the Observatory at Hartwell House.

By John Lee, Esq., LL.D., F.R.S., &c.

"In presenting an account of my observatory to the Astronomical Society, it is not my intention to enter into any lengthened detail, as both the building itself and the principal instruments which it contains are minutely described by my excellent friend Admiral Smyth in the *Ædes Hartwellianæ* and in the *Celestial Cycle*. The former work, which was printed for private circulation only, contains an account of my first small observatory with its fixed and portable instruments. Here the tangible advantages of practical astronomy followed the physical theory, which formed a prominent part of my college reading; and the pleasures I have thus experienced, of all others perhaps the purest we are permitted to enjoy, have induced me to associate with myself as astronomical fellow-labourers my friends and neighbours at Aylesbury, Hartwell, and Stone. The three observatories which have recently been erected in these localities have already been described at the meetings of this Society; and to complete the cycle, I wish to offer the following short description of my own.

"My second and larger observatory adjoining the south-east angle of Hartwell House, consists of an ante-room, transit-room, and equatoreal tower, in the erection of which no expense was spared in order to insure the approbation of the most critical astronomer. Admiral Smyth, the Chairman of our synod, availed himself of the advice and labours of the elder Tully, Mr. Dollond, Mr. Vulliamy, Mr. C. May, and Mr. Thomas Jones, and, therefore, in the construction of both the building and the instruments he was determined to be satisfied with nothing short of the utmost attainable perfection. That the work of the practical optician and engineer was sufficiently successful, is evident from the meed of praise which it has received in the *Ædes Hartwellianæ*, as well as from the fact especially recorded by the Admiral himself, that he was generally enabled to give to his Hartwell measures of double stars *high weights*.

"The model now exhibited of the observatory, by our village carpenter, Mr. G. Carter, is on the scale of an inch to the foot,

points out in detail the position and size of the instruments. The ante-room is 11 feet by $8\frac{1}{2}$ feet. The transit-room is 16 feet 5 inches by 11 feet 2 inches, and 10 feet 5 inches in height; the tower $15\frac{1}{2}$ feet in diameter, and 16 feet in height. The north pier in the tower is a single block of Portland stone, and is 12 feet 3 inches high and 3 feet square at the base. The polar axis, which is 10 inches square in the centre and 8 inches at the two extremities, is 13 feet long. The piers are built upon a bed of concrete, which is placed upon the limestone rock at a depth of seven feet below the surface. The walls are 14 inches thick and covered with a thick coat of cement.

"The transit instrument 5 feet focal length, with an object-glass $3\frac{1}{4}$ inches in diameter, was supplied by Mr. T. Jones, of Charing Cross, at a cost of 105*l.* It is supported by two piers of Bath stone, and borne at the centre by two well-proportioned cones, which terminate in rigorously turned pivots of bell-metal of $1\frac{1}{4}$ inch in diameter. These pivots, which are 30 inches apart, rest upon Y's, also of bell-metal, but the latter are additionally hardened by the introduction of a thin slice of Brazil pebble. The adjustment of the transit instrument is tested by two meridian marks, which are placed respectively 100 feet north and south of the observatory slit, and are inserted into blocks of marble—that on the north side representing the Temple of Janus as given on a large brass medal of Nero, and that on the south side being a miniature of the façade of the Temple of Concord at Girgenti.

"The excellent clock by Vulliamy, with Graham's dead-beat escapement, cost also 105*l.*

"An object-glass for the equatoreal was purchased from Sir James South for 200*l.*, but on the dismantling of the Bedford Observatory, Tulley's *chef-d'œuvre*, 5·9 inches in diameter and 10 feet focal length, which had been purchased by Admiral Smyth from Sir James South for 220*l.*, was transferred to Hartwell with its equatoreal mounting by Dollond, and its equatoreal clock invented by the Rev. R. Sheepshanks, and re-made in its essential parts by Mr. C. May. This fine instrument has been duly described by Admiral Smyth in his *Cycle of Celestial Objects*, and in the *Memoirs* of the Society.

"It would be difficult from lapse of time to calculate the expenses of the whole Observatory, and, perhaps, I should feel unwilling to look at its amount. But I may say for the encouragement of younger amateurs, that many a sportsman expends more on his 'Joe Manton' and his dogs, in order to observe the altitude and azimuth of a brace of birds, than a practical astronomer *need lay out* in a most efficient battery for securing the position and distance of double stars.

"The niches in the Observatory are ornamented with busts of Newton, Laplace, and Mrs. Somerville, and portraits of eminent astronomers adorn its walls.

"I may add that meteorological observations are made daily with a complete set of instruments, which were approved by James

Glaisher, Esq., the indefatigable Secretary of the British Meteorological Society, and the monthly returns are sent to the Registrar General."

Dr. Lee appended to the foregoing account *A Genealogical Table of the Descendants of the Bedford Observatory*. It appears from this Table that the Bedford Observatory was erected in the year 1827; and that it has been followed in direct succession by the construction of ten or eleven other observatories throughout the country.

Mean Places, as determined by Meridian Observations at the Cambridge Observatory, of the Stars of Comparison used by Mr. Maclear at the Cape of Good Hope in Observations of Comet II. 1853. (*Schweizer's Comet.*)

See Monthly Notices, Vol. xiv., No. I., p. 8.

Ref. No.	Name of Star.	Mean R.A. of Star Jan. 1, 1854. h m s	No. of Transit Obs.	Mean N.P.D. of ✱ Jan. 1, 1854. ° ′ ″	No. of Circle Obs.
1		4 59 52·32	2	103 19 20·63	3
2	H.C. 11551	5 57 33·44	2	104 4 57·36	5
3		5 57 55·39	3	104 1 38·67	3
4		5 58 14·21	2	104 6 49·58	3
5		6 11 56·91	3	104 18 19·60	3
6		6 12 48·78	2	104 16 43·45	3
7		6 14 35·97	3	104 14 27·82	4
8	H.C. 12825	6 32 36·39	3	104 1 5·18	4
9	B.A.C. 2221	6 40 11·56	3	104 16 22·08	3
10		6 42 11·88	3	104 13 44·49	3
11		6 47 39·40	3	104 3 48·17	3
12	H.C. 13392	6 48 6·38	3	103 58 24·34	2
13	μ Canis Majoris	6 49 25·22	3	103 51 28·46	3
14		6 56 23·24	2	103 57 31·47	3
15		6 58 19·26	3	103 45 4·95	3
16	H.C. 13848	7 0 41·58	3	103 49 38·05	3
17		7 1 53·54	3	103 45 37·84	3
18		7 8 41·26	3	103 32 54·11	4
19		7 10 3·02	2	103 25 28·99	3
20	H.C. 14228	7 11 14·87	3	103 37 47·73	2
21		7 11 19·93	2		
22		7 14 26·97	2	103 21 28·21	3
23		7 14 51·30	2	103 11 37·77	3
24		7 16 38·61	3	103 4 18·22	3
25		7 19 49·56	3	102 53 59·92	3
26		7 21 39·14	3	102 39 39·14	3
27		7 22 32·77	3	102 50 27·85	3
28		7 23 54·12	2	102 27 48·78	3

Cambridge Observatory, June 14, 1854.

J. Challis

The Comet of April (*Comet II.*) 1854. By Eyre B. Powell, Esq. F.R.A.S.

"On the evening of the 4th April last my attention was caught by a star-like object in the west, badly defined, and with a short, indistinct ray running upwards. On turning a $3\frac{1}{2}$-foot telescope upon the object, it was at once seen to be a comet. Time did not allow me to measure the right ascension and declination of the stranger that evening; but, on the 5th and succeeding nights, up to the 28th, with one or two breaks, I managed to secure observations more or less trustworthy. These are given in the following table; and they were taken with a 5-foot refracting telescope by Messrs. Simms, mounted equatoreally in a small octagonal room, erected for the reception of the instrument, within the grounds attached to the Government Native College. This room is exactly the counterpart of the one at the Madras Observatory, and was constructed under the superintendence of Captain Jacob, the Hon. East India Company's Astronomer. The object-glass of the telescope is 4 inches in diameter, and performs very satisfactorily; the magnifying power generally applied was about 55; but a higher power was used at first. On the 5th, 6th, and 7th April, I obtained the right ascension and declination of the comet directly from the readings of the equatoreal, the hour-circle of which measures seconds of time, and the declination-circle minutes of space. The errors of adjustment were found by comparing the instrumental right ascension and declination of a known star, having the same hour-angle and nearly the same declination as the comet, with the catalogued values of those quantities. On the remaining nights the position of the comet was ascertained by taking differential observations of it with one or more stars. At first the comparison stars were selected from the British Association Catalogue; but, towards the end of April, I was obliged to fall back upon Weisse, no large stars lying about the comet's path.

Observations (*affected with Parallax and Aberration*).

Day. 1858.	Madras Sid. Time. h m s	R.A. h m s	Decl. ° ′ ″
April 5	7 56 30	2 42 38	(+) 15 30 50
6	7 58 57·5	2 54 23·5	14 31 52
7	8 14 46	3 5 24	13 31 53
8	7 54 17	3 15 31	12 32 9
9	8 6 51	3 25 14	11 30 34
10	8 25 53	3 34 18	10 30 33
11	8 16 23	3 42 40	9 33 3
	8 49 36	3 42 51	9 31 39
12	8 13 43	3 50 29·5	8 36 42·5
13	8 53 0	3 58 4·4	7 40 54
15	8 34 10	4 11 23·3	5 58 59
16	8 49 40	4 17 34·6	5 10 12

Day. 1853.	Madras Sid. Time. h m s	R.A. h m s	Decl. ° ′ ″
April 18	9 4 55	4 28 48·6	(+) 3 39 38
19	9 6 24·5	4 33 58	2 57 36
22	9 28 38·7	4 47 55	1 1 37
23	9 18 25	4 52 10	(+) 0 26 19
24	9 29 7·5	4 56 14	(−) 0 7 33
26	9 33 59	5 3 46	1 10 52
27	9 47 46	5 7 19·6	1 40 51
28	9 45 24·4	5 10 45	2 9 32

Number of Comparisons or Direct Observations.

April 5. One direct observation.
6. Three direct observations.
7. Three direct observations.
8. One direct observation, and one comparison with B.A.C. 1087.
9. One direct observation, and two comparisons with 4 *Tauri*.
10. Three comparisons with B.A.C. 1174.
11. Four comparisons with Weisse iii. 814.
Three comparisons with B.A.C. 1180.
12. Two comparisons with B.A.C. 1295 and 1304.
13. Four comparisons with B.A.C. 1296.
15. Four comparisons with Taylor 1468 and 1499.
16. Six comparisons with Weisse iv. 361, 506, and 554.
18. Eight comparisons with Weisse iv. 608.
19. Seven comparisons with Weisse iv. 754.
22. Five comparisons with two small stars, which were again compared with *β Eridani*.
23. Three comparisons with Weisse iv. 1207.
24. Three comparisons with Weisse iv. 1346.
26. Three comparisons with Weisse v. 129.
27. Five comparisons with Weisse v. 169.
28. Two comparisons with Weisse v. 419.

"On the 5th the outline of the envelope was sharp; the length of the comet measured about 22′; and the nucleus was pretty bright, possibly equal to a star of the 3.4 magnitude.

"On the 6th I estimated the diameter of the nucleus at 8″; it was ragged, and equal to a star of the 4th magnitude. The tail was about 30′ long by measurement; also I fancied it not so broad as on the 5th.

"On the 7th the views I obtained were rather indistinct, in consequence of some clouds drifting across the western sky from south to north; but, with the exception of the nucleus appearing somewhat larger, the comet presented nearly the same aspect as on the 6th.

"On the 8th, owing partly to the moon, now on the increase and beginning to 'shine down' the faint light of the comet, the outline of the envelope was indiscernible. The nucleus was about as bright as a 6th magnitude star.

"On the 12th a small star was visible through the tail.

"On the 18th the comet would not bear any illumination, and the transits were taken on the lower edge of the micrometer comb. I trust that the care expended on the observations compensated in

a great degree for the imperfection naturally belonging to the mode of observing. The tail was longer, perhaps about 1° in length.

"On the 28th the comet was so excessively faint that the observations were taken with the greatest difficulty. In fact, it was no more than a small, whitish, cloudy patch; still there was something like a faint star as a nucleus: this was especially noted as the comet passed through the zero hole of the comb.

"Taking the observations of the 8th, 13th, 18th, and 27th, I proceeded to calculate the orbit.

"To obtain the perihelion distance and the time of perihelion passage, I used Pontécoulant's modification of La Place's method, given in the Appendix to tome ii. of the *Théorie Analytique du Système du Monde*. The following are the chief data and results; the notation corresponding to that of Pontécoulant:—

Epochs, Greenwich M.T.	Longitudes Observed.	Latitudes Observed.
April 8·06067	49° 50′ 56″ ($a°$)	5° 22′ 24″ ($b°$) (−)
13·08769	59 0 3 (a)	12 33 19 (b) (−)
18·08229	66 1 16 (a')	17 55 12 (b') (−)

$$\frac{da}{dt} = +1{\cdot}63593$$

$$\frac{d^2a}{dt^2} = -4{\cdot}884596$$

$$\frac{db}{dt} = -1{\cdot}269075$$

$$\frac{d^2b}{dt^2} = +4{\cdot}17352$$

$$r^2 = 1{\cdot}00692 - 1{\cdot}62997 \cdot \varrho + 1{\cdot}0496 \cdot \varrho^2$$

$$\frac{1}{r} = {\cdot}496558 - 1{\cdot}8551335 \cdot \varrho + 2{\cdot}941991 \cdot \varrho^2$$

$$\frac{\delta\varrho}{dt} = + {\cdot}919686\,\varrho$$

"Assuming $r = {\cdot}65$, and $\varrho = {\cdot}99$, as suggested by trials,

$$\delta r = -{\cdot}00070196 \text{ and } \delta\varrho = -{\cdot}0008311$$

$$\therefore\ r = {\cdot}649298 \quad \text{and} \quad \varrho = {\cdot}9891689$$

"Using these values in approximating,

$$\delta r = +{\cdot}0000020 \text{ and } \delta\varrho = -{\cdot}0000007$$

Hence $r = {\cdot}6493$ and $\varrho = {\cdot}989168$

Also $$r \cdot \frac{dr}{dt} = +{\cdot}868954$$

"Therefore D, the perihelion distance = ·27176, and the time of perihelion passage = March 24·0028, v being 99° 22′ 34″.

"Then, by comparing the differences of the anomalies, afforded first by the parabolic theory *per se*, and secondly by it and the observations combined; and by tracing the effects upon those differences of variations in the above fundamental elements, I found, after about four approximations,

$v - u = -(1' - 19'')$ for the error of anomaly between the 8th and 13th
and $v' - u' = +(4' - 12'')$ — — — 8th and 27th

"The above errors might be almost eliminated by varying slightly the time of perihelion passage; but, without taking aberration and parallax into account, further calculation would appear to be labour thrown away.

"The corresponding elements are as follow:—

"*Elements.*

Time of Perihelion Passage, March 24·0501

Perihelion Distance	·27975
	° ′
Longitude of Perihelion on Ecliptic	171 21
Longitude of Ascending Node	316 28
Inclination of Orbit	81 20

Motion retrograde.

"Hence it seems that the comet is not one whose elements have been already ascertained. There is, however, a considerable approach to agreement between some of its elements and the corresponding ones of a comet observed by Hevelius in 1677, the orbit of which was calculated by the celebrated Halley.

"Thus:—

	Motion.	Perihelion Distance.	Inclination.
			° ′
Comet of 1677	Retrograde	·28059	79 3
Comet of 1854	Retrograde	·27975	81 20

"*Madras, May 16th*, 1854."

Elements of Proserpine, for the Opposition in 1854.

Computed by Mr. George Rümker, Observer at the Observatory, Durham, from the following four normal places, deduced from all the recorded observations at her last appearance:—

1853.	R.A.	N.P.D.	
	° ′ ″	° ′ ″	
May 20·5	205 10 21·8	99 49 5·5	from 27 observations.
June 3·0	204 3 20·1	99 47 55·9	— 14 —
27·0	204 12 20·1	100 13 41·2	— 9 —
July 6·0	206 25 23·7	101 28 7·9	— 14 —

M	349 16 41·9	1853, June 0·0
π	235 24 35·7	1853·0
λ	45 55 18·0	
i	3 35 45·0	
ϕ	4 55 51·1	
Log a	0·4236444	
Log μ	2·914541	

The following are the errors (computed—observed) in longitude and latitude:—

May	20·5	−0″·4	+1″·2
June	3·0	−0·1	−1·0
	17·0	+0·3	−1·3
July	6·0	+0·6	+1·0

The errors in R.A. and N.P.D. of two observations at Washington, on the 23d and 24th of July, not included in those from which the elements are obtained, are

		∂a	∂p
July	23	+2″·8	−0″·2
	24	−2·5	−1·7

The foregoing elements were accompanied with an ephemeris of the planet for the opposition of 1854, extending from July 15 to August 24. A few printed copies have been forwarded by Prof. Chevallier to the Apartments of the Society for distribution.

Notes on the Second Comet of 1854. By the Rev. T. W. Webb.

"As far as my opportunities of information have extended, this fine and conspicuous comet appears to have been noticed at Ross, in the county of Hereford, on the evening previous to its general recognition by English astronomers. About $7^h\ 30^m$ P.M. on the 28th of March, it was pointed out by an intelligent little boy of ten years old as "a stranger in the sky," and in consequence was seen by several persons before it sank into a bank of clouds in the N.W. horizon. From one of these, my friend, Mr. W. H. Purchas, I received immediate information of its appearance, and was thus enabled to watch its progress every night except two, from March 29th to April 5th, with a good achromatic by Tulley, of 5½ feet focal length, and 3·7 inches aperture. In this and smaller glasses, as well as to the unassisted eye, it uniformly appeared in all its parts of a pale orange or fiery hue, and of an aspect which would have been deemed in former times terrible and portentous.

"During the earlier observations, with low powers, and comparatively near the horizon, the brilliant nucleus, of several seconds in diameter, was seen suddenly but not sharply defined, having

neither a stellar nor planetary appearance. With higher magnifiers its vivid light was diffused into a hazy mass of considerable brightness, not much condensed towards the centre. April 2d, it was thought smaller and more stellar with a power of 64. April 3d, this impression was confirmed; with 64 and 80 its aspect was almost that of a star; with higher powers up to 330, there was occasionally a suspicion of a strong luminous point in the centre of the diffused nebulous brightness; but this was seldom visible, and the air was too unsteady to admit of any certainty. In this respect it reminded me of the comet of July 1850, as well as of similar observations of Hooke and Schröter. With powers from 80 to 330, the light of the nucleus appeared condensed or accumulated, though but very slightly, on the side next the sun: this was also noticed on the following night, April 4th. April 5th, the nucleus was thought to be still becoming smaller and more definite. At the end of March it was as conspicuous to the naked eye as a 2d or 3d magnitude star, but it soon became less luminous.

"On March 29th, the nucleus was encompassed by a narrow border of faint yellow light, forming a hemispherical cap or envelope on the side turned to the sun, which seemed to have become less distinctly marked by April 3. From this envelope rose the tail, which was very narrow in proportion to its length, widened very gradually, and exhibited a decided curvature. Its convex side seemed a little better defined than the other on March 29th; this difference, however, on April 3d appeared to have become less sensible. Its length, notwithstanding a brilliant moonlight, was estimated on April 4th at least 5° by oblique vision with the naked eye. On March 30th, I began to perceive, with a comet eye-piece of M. Sécrétan's construction, that it ascended from the sides of the envelope in two apparent streams, very little denser, however, than the general mass, and soon becoming undistinguishable from it. April 4th, this indication of a hollow interior was perhaps still more evident, and the stream on the N. side seemed the more luminous, though on the following edge of the train as respects the orbital motion of the comet.

"On the first evening of my observations, as well as frequently afterwards, I thought there was, to the naked eye, aided only by a concave lens to correct near-sightedness, an appearance of blazing up, or flashing, issuing from the head into the denser portion of the tail; of this Mrs. Webb, whose eye is very sensitive to light, was on several occasions perfectly confident. These coruscations could not however be traced in the telescope.

"Mr. Purchas, at Ross, observed nearly the same phenomena with an achromatic of 4 inches aperture, as well as with the naked eye. At its first appearance on March 28th, he considered the nucleus as brilliant as *β Arietis*, with a tail extending about 3° in the strong twilight. He noticed the sharper definition of the convex side of the tail at first, as well as its subsequent decrease in this respect: its length was estimated by him on March 29th at [illegible] and on March 30th he thought he could detect, without

faint traces of two smaller and shorter tails, one on each side, diverging at a greater angle from the nucleus, and resembling some of the appearances figured by Schröter in 1811. This, however, was not confirmed by the telescope, nor did he see it on the following nights. He traced the same faint indications of a hollow structure in the train, especially on March 30th. On April 3d, and subsequently, they had become less evident. Coruscations appeared to him to be distinguishable both by the naked eye and with the telescope."

Photographic Pictures of the Moon taken with the Equatoreal of the Liverpool Observatory. By John Hartnup, Esq.

Mr. Hartnup exhibited ten collodion pictures of the moon, of 1·35 inches diameter, and ten enlarged copies, taken from the originals, which varied from 2 to $4\frac{1}{2}$ inches in diameter.

The photographs were all taken during the preceding month; and Mr. Hartnup stated that he only looked upon the progress now made as one step towards that degree of perfection which he had every reason to hope he should shortly arrive at.

In taking the pictures, the clock was so regulated that the telescope followed the moon in right ascension nearly; then, the hour-circle and the declination-circle being both clamped, the image of the moon was kept as nearly as possible in the same place, as seen in the finder, by means of the rods for giving slow motion to the instrument in right ascension and declination. The circles are each four feet diameter; the slow motion is given by means of tangent screws; and the instrument is so firmly mounted as to render the task of correcting for irregular motion of clock, change of moon's declination, and change arising from differences of refraction and parallax during the operation, comparatively easy.

The chemicals employed were those ordinarily used for taking a positive collodion picture on glass; and Mr. Hartnup acknowledged the great obligation that he was under to Messrs. Forrest, Berry, Edwards, M'Innes, and Towson, of the Liverpool Photographic Society, for the kind advice and assistance which he had from time to time received from them.

Of the photographs exhibited there were two (one taken before and one after the full), which, when placed in the stereoscope so as to be seen as one image, the appearance was that of a semi-transparent globe, the effect being perfectly stereoscopic. The enlarged pictures were shown on a screen by means of a magic lantern, kindly supplied for the occasion by Mr. Simms. Most of them were too large for the lens, so that only a portion of the picture could be seen at the same time on the screen, though it was eight feet diameter. There was, however, one photograph of a half moon, and one of a full moon, small enough for the screen; and these pictures were much admired. The President alluded to the gratifying progress of Mr. Hartnup's labours in connexion

with this interesting subject. At the same time he took occasion to point out the imperfect definition of the inequalities on the bright limb of the moon, and remarked that this was a defect of photographic representations, which placed them in strong contrast with the results of direct observation. To devise adequate means for remedying this defect was one of the principal objects to be aimed at in such experiments, and it was an essential element of success that there should be a contrivance for applying an equable and smooth motion to the surface destined to receive the photographic impression.

Mr. Burr states, in reference to his observation of the occultation of Mars inserted in the last *Monthly Notice*, that the estimated longitude of his observatory is $23^{s}\cdot8$ W.

Account of Observations of the Total Eclipse of the Sun of November 30, 1853, *made at Ocucaje, in Peru.* By M. Carlos Moesta.*

(*Communicated by Admiral W. H. Smyth.*)

The author of this account, who is Director of the National Observatory at Santiago, Chili, was appointed by the Chilian Government to proceed to the neighbouring republic of Peru for the purpose of making observations of the total eclipse of the sun which was to occur on the 30th of November, 1853.

By a graphic delineation, based upon the results given in the *Nautical Almanac*, M. Moesta found that the central line of the totality of the eclipse would enter the coast of Peru from the Pacific at a point situated in long. 76° 2′ west, and lat. 14° 27′ south. Pisco was the nearest town to this part of the coast, but M. Moesta resolved to observe the phenomenon at some convenient place in the interior, elevated as much as possible above the level of the sea.

The various instruments required for observing the phenomenon were obtained from the Observatory of Santiago. Among these was an achromatic refractor of $4\frac{1}{2}$ inches aperture and $5\frac{1}{3}$ feet focal length, by Frauenhofer, furnished with a reticulated micrometer adapted for measuring differences of right ascension and declination, and also angles of position. Another telescope, a refractor, by Dollond, of $2\frac{3}{4}$ in. aperture and $3\frac{1}{2}$ feet focal length, was used by M. Lira, the assistant of the Observatory, who accompanied the author in his expedition. Besides these two instruments, the Observatory furnished for the observation of the phenomenon a reflecting circle by Ertel, divided to every 10″, fitted with two

* This Report was originally drawn up by M. Moesta and presented to the Minister of Public Instruction of the Chilian Republic.

verniers, a sextant by Pistor, a good chronometer by Parkinson and Frodsham, a barometer, and various thermometers.

The author embarked at Valparaiso on the 1st of November, and arrived at Pisco on the 9th of the same month. By means of ten altitudes of the sun, taken on the 11th, he found that this town was situate $22^m\ 12^s{\cdot}78$ west of the meridian of the Observatory of Santiago. On the 13th of November, while still residing at Pisco, the author carefully examined the sun's disk with the view of ascertaining whether any spots or faculæ were visible which might serve to throw light upon some of the phenomena of the approaching eclipse. Two spots were visible near the eastern margin of the disc, one of which consisted of a group of smaller spots united together like the links of a chain. These spots continued to be seen till the 20th of the month, but no other spot had appeared on the eastern margin of the disc during the interval of their being visible. It is to be remarked, also, that these could not have remained on the disc till the time of the eclipse.

Finding that Pisco was situate in an unfavourable locality for observing the eclipse, the author removed from that town on the 23d, and arrived on the 25th at Ica, which from an old Spanish map he concluded to lie in the central line of the shadow. He found, however, from observations made on the evening of his arrival that the latitude of Ica was in reality only 14° 5′, and that it was situate $1^m\ 56^s$ to the east of Pisco. It was accordingly necessary to advance about eighteen miles farther towards the south. On the evening of the 27th M. Moesta therefore pursued his journey in that direction, bearing a little towards the east, and on the following morning arrived at Ocucaje, a place which from actual observation he found to be situate in lat. 14° 21′, and to be 20^s east of the meridian of Ica. He consequently found himself within about half a mile of the central line of the shadow.

M. Moesta selected for the place of observation an eminence, which, from barometrical measurement, he found to be 880 feet above the level of the sea. On the 28th, he observed a series of corresponding altitudes of *α Eridani* and *α Argus*, from which the latitude appeared to be 14° 21′ 21″. On the morning of the 30th, the sky was everywhere overcast, but about 8 o'clock the sun made his appearance, and the heavens gradually cleared up, promising to afford a full view of the eclipse to the numerous spectators who had congregated to this sequestered spot for the purpose of witnessing so unusual a phenomenon.

All the necessary preparations were now made for observing the eclipse. The error of the chronometer was determined by several altitudes of the sun. The telescopes were placed in appropriate positions. It was arranged that M. Lira should note the variations of the barometer and thermometer; Mr. Clark, an intelligent traveller from North America, undertook to observe all the phenomena which might be visible to the naked eye.

The following account of the appearances observed during the eclipse is thus given by the author:—

"A few hours before the commencement of the eclipse, I examined the disk of the sun with the Frauenhofer refractor, in order to ascertain whether any spots were visible, and to fix their position if there should be any; but all my efforts were unsuccessful. There was not the slightest spot or interruption of the surface, and the disc appeared perfectly round. I make particular mention of this circumstance on account of its importance as regards the red protuberance seen during the total obscuration, since it has been generally supposed that some relation exists between these phenomena and the solar spots. I shall presently have occasion to speak of this protuberance.

"Two minutes before the commencement of the eclipse, I directed my telescope to the point of the margin of the sun where it was expected that the moon would make her exterior contact. At $2^h\ 13^m\ 25^s{\cdot}5$ mean time of the place of observation, I perceived the first contact of the two discs. No extraneous phenomenon appeared to accompany this occurrence. The moon gradually advanced over the sun's disc; her projection upon the latter tracing out a dark and well-defined arc. On the southern margin of the moon, many elevations were visible, but none of them was sufficiently prominent to admit of ascertaining its height. At $2^h\ 52^m$ the whole western part of the horizon had cleared up. At $3^h\ 8^m$ the small clouds in the vicinity of the sun had disappeared, but the horizon towards the north and east still remained covered with dense clouds. At this time the wind from the south blew with less violence, and the cold began to make itself sensible. The landscape and surrounding objects had changed their colour, and everything seemed to announce the approach of the decisive moment. At $3^h\ 32^m$, while fixing my attention on the eastern part of the sun's disc, where the uncovered space gradually became narrower and narrower, while the two limbs of the sun and moon were perfectly distinguishable, a nebulous substance of a reddish colour, apparently in a state of ebullition, all of a sudden filled up the space between the two limbs. This substance appeared to be traversed by a vein of colour more obscure. It continued to accumulate as the space between the two limbs became narrower, as if it had been forced up between them. Finally, it acquired a ruddy colour of so dark a tinge that it became difficult to distinguish clearly the two limbs. The contact of the latter took place at $3^h\ 32^m\ 15^s{\cdot}5$; at the same instant the last ray of the sun and the last trace of the nebulous substance vanished. From this moment the eclipse presented a very imposing aspect, nature appearing in all her majesty.

"The moon, whose diameter was then 82″ greater than that of the sun, resembled a round and perfectly black circle, and to the naked eye appeared extremely well defined at its circumference. Around it was visible a luminous ring called a *corona of light*, whose soft lustre had a colour between white and azure, like the light of a gas-burner. The light of the ring was as bright, at the part where it touched the moon, as the light of the full moon in a

clear night at Santiago; but its intensity diminished rapidly, insomuch that, at a distance equal to a fourth part of the lunar diameter, it confounded itself with the pale light of the sky. Conceiving for a moment a vertical circle passing through the centre of the moon, the ring will then divide itself into two parts; viz. one situate to the south, and the other to the north, of the circle in question. Almost all the northern part of the ring was uniform; the opposite part, however, was composed of numerous rays which appeared to issue from the ring, and which all seemed to have the same length with the exception of two very large rays. With respect to the latter, one of them was perceived to shoot upwards, inclining at an angle of 20° to the south of the vertical, and according to estimation extended to a distance equal to the diameter of the moon. The other issued downwards, not diametrically opposite to the former, but inclined about 10° to the north of the vertical, and was rather shorter. The aspect of these two rays very much resembled the tail of a comet, which was narrower at the extremity than at the nucleus, and which was very radiant in all its structure, since they manifestly did not emit a uniform light, but were composed of numerous small rays.

"Immediately after the sun was entirely concealed from view there was visible, in the northern part of the corona of light, a rose-coloured protuberance resembling a cloud, which seemed to issue from the margin of the moon. Its form, which was longitudinal, and also its initial position, are indicated in the sketch.* The colour of this protuberance was not uniform, but was evidently darker in some parts than it was in others. The form continued the same, but as regards its position, I thought I could be certain that it moved towards the ray which shot upwards from the *corona of light*. Gradually its colour became fainter, and after the lapse of about $1^m\ 20^s$ from its first appearance, the whole of the protuberance had vanished. With a position-micrometer I measured its height above the margin of the moon, which I found to be 1′ 8″.

"On the same side of the *corona* there were seen during the whole time of the obscuration two dark spots, not round, but rather of a square form, and attached to the margin of the moon. It seemed as if there had been an interruption of the ring in that part, or rather two apertures through which the dark ground of the heavens was visible. This could be no optical illusion, since many persons were present who could discern the spots referred to perfectly well with the naked eye. Their height above the margin of the moon was estimated at 1′. * * * * *

"The corona disappeared instantaneously with the reappearance of the first ray of the sun, since neither before nor after the total obscuration was the slightest vestige of it to be seen: there was also a similarly sudden change in the brightness of the atmosphere. So long as even the smallest part of the solar disc remained uncovered, it was possible to perceive surrounding objects almost as well as on an

* The drawing here referred to has not been communicated to the Society.

ordinary day, except that every object assumed a peculiarly pale hue. At the moment of the sun being wholly concealed from view, there suddenly came on a kind of darkness very different from that which usually characterises the evening twilight. Towards the horizon, and over the whole north and east where the sky continued cloudy, the darkness of the atmosphere resembled night; while, on the other hand, in the neighbourhood of the moon it was much less intense. The degree of obscurity may be judged of from the following observations:—the planets *Venus* and *Jupiter* were seen in all their brilliancy; *Antares*, which was within 5° of the sun, and was consequently in the immediate vicinity of the corona, was distinctly visible to the naked eye; and as that star is between the first and second magnitude, it might be generally asserted that the degree of obscurity was such as to allow of stars of the second magnitude being seen. There was not sufficient time to look for other stars.

"I had beforehand placed the first page of the *Nautical Almanac* near the telescope, and during the total obscuration I could read it very distinctly at the distance of two or three feet. M. Lira could read the scale of the thermometer without the least difficulty.

"The total obscuration lasted $2^m\ 59^s$, since it ended at $3^h\ 35^m\ 14^s{\cdot}6$.* *Jupiter* remained visible some minutes afterwards, and *Venus* shone brilliantly till $4^h\ 3^m$, that is to say, twenty-eight minutes after the total obscuration." * * * *

The author remarks, that the researches on the physical constitution of the sun seem to indicate the existence of a dark body enveloped in two atmospheres, the interior being non-luminous and somewhat resembling our own atmosphere, while the exterior one, or photosphere, is the exclusive source whence emanates the solar light. Referring, then, to the observations of the total eclipses of the sun of 1842 and 1851, he thus proceeds:—

"All these observations render it probable that the sun has a third atmosphere, imperfectly diaphanous, enveloping the photosphere, and that through the apertures of the spots there issue gaseous exhalations, which, rising to the third atmosphere, form there the coloured clouds, constituting the observed protuberances.

"Thus it arises that our knowledge relative to the physical constitution of the sun depends chiefly upon exact observations of total eclipses of that body, and it is, consequently, of importance that we take advantage of such eclipses as successively occur, in order to improve and amplify the little that we know upon that subject.

"Turning now to the observations made in Peru, I have to remark, that there was visible during the total obscuration a red protuberance, although a corresponding spot on the sun's disc was

* The author states in another part of his account that the eclipse terminated at $4^h\ 43^m\ 41^s{\cdot}8$.

not afterwards perceived. *It hence results that the supposed connexion between the spots visible on the solar disc and the red protuberances is destitute of foundation*, and that the so-called protuberances are in reality gaseous exhalations, which becoming visible in the third atmosphere, must have issued, during the eclipse of the 30th of November, through very small apertures of the photosphere. Such an hypothesis is by no means improbable, since there is always discernible on the solar disc, by the aid of powerful telescopes, a net-work of pores subject to perpetual change.

"The short duration of the protuberance, joined to its change of colour and its apparent motion, are circumstances which seem to indicate the imperfect transparency of the third atmosphere, as well as whirling currents of wind which occur in it, analogous to those which prevail in the terrestrial atmosphere.

"The corona of light has been seen during several former eclipses, but, as far as I am aware, in no instance with such brilliancy, or with such a peculiar form, as on the present occasion. The radiant light of the corona, throughout all its southern side, strongly resembled the aurora borealis; but the two large emanations of light, above referred to, exhibiting the radiation upon a much larger scale. The contrast between this part of the corona and the northern part, whence a soft light, unaccompanied by rays, appeared to emanate, imparted a singular aspect to the phenomenon. I am of opinion, that the latter may be satisfactorily explained by supposing the activity of the photosphere, not to be equal in all its parts, but that by some cause it is more violently agitated in certain places than in others. It is not so easy to give a satisfactory explanation of the two dark spots above mentioned. I refrain from making any remarks upon the extreme transparency of the terrestrial atmosphere in this part of Peru. The foregoing observations sufficiently attest this fact."

Meteorological and other Observations made during the Eclipse.

In order to ascertain with greater precision the influence of the eclipse upon the thermometer, M. Moesta caused observations with two thermometers to be made, not only on the day of the eclipse, but also on the preceding and following days. One of these thermometers was placed in the shade; the other, which had a blackened bulb, was exposed to the direct influence of the sun's rays. The author then gives the details of the observations on each day. It appears from them, that on the 30th of November, during the time comprised between the commencement of the eclipse and the total obscuration, the thermometer exposed to the sun, sank $22^{\circ}{\cdot}5$ Fahrenheit, after which it continued to rise till $4^{h}\ 15^{m}$, attaining, in this interval, an elevation of $3^{\circ}{\cdot}8$. On the other hand, the same thermometer was observed on the 29th to descend $5^{\circ}{\cdot}2$ during

the former of these two intervals, and *continued to descend* 6°·7 during the latter interval. On the 1st of December the descent, during the former interval, was 9°·7.

"A change of 14°·4 in the temperature during little more than an hour ought certainly," says the author, "to make itself sensible; and the truth is, that during the time of the total obscuration I was shivering with cold." This sudden fall of the temperature was accompanied by an interesting phenomenon, which manifested itself under very striking circumstances. At the entrance of the shadow on the coast of Peru, the air, usually so hot from the sandy plain upon which it rests, suddenly condensed from the effect of the eclipse, and this caused a current of cool air to flow from the ocean for the purpose of restoring the equilibrium of the atmosphere. There ought from this cause to have been a wind from the west; however, as a cool breeze from the south had continued to blow during the whole day, there was a violent meeting together of two distinct currents, and the consequence was, that during the total obscuration a succession of whirlwinds occurred which in some instances raised the sand to a height of 200 feet.

Another phenomenon strongly attracted the attention of the author,—a species of cloud of a pale azure colour, mingled with purple. To the south of his position, at the distance of half a league, a table-mountain was situate, from which this cloud appeared to ascend in columns at three o'clock in the afternoon. It continued to augment till the time of the total obscuration, when it presented a very dense and black aspect. It then gradually grew less, and, finally, disappeared a little before the termination of the eclipse.

M. Moesta also remarked that, during the total obscuration, the leaves of the carob-tree closed up as if it were night, and resumed their natural position shortly after the reappearance of the sun. In the case of the leaves of the cotton-tree, Mr. Clark noticed a similar change. The latter also remarked that, during the total obscuration, all sorts of insects disappeared, that the fowls grouped together, and that other species of poultry continued flying about very near the ground. "Great," says the author, "was the impression which the occurrence of so many rare phenomena during the total obscuration produced on the minds of the spectators, who all saluted with enthusiasm the first rays of the sun on the occasion of his reappearance."

Remarks on the Early History of the Researches of Astronomers relative to the Spheroidal Figure of the Earth. By the Editor.

The establishment of the spheroidal figure of the earth must ever be regarded as one of the noblest results of modern scientific inquiry. It must be acknowledged that a good deal of misconception has hitherto prevailed respecting the original researches connected with this important problem of mechanical astronomy. It has been generally admitted by writers on astronomy that Newton and Huyghens first demonstrated by strict mathematical reasoning that, under certain conditions as regards the mutual action of its constituent particles, the figure of the earth would be spheroidal, the equatoreal being to the polar axis in a certain assignable ratio, which each of these geometers computed according to his peculiar view of the physical conditions of the problem. In this inquiry, however, the priority in respect to time has been, in some instances, awarded to the labours of Huyghens, upon the evidence of a passage contained in his "Dissertation on the Cause of Gravity." Delambre, who was at first led to give currency to this view of the subject, had, subsequently, the merit of proving it to be inconsistent with the records of history. From an examination of the early registers of the Royal Academy of Paris and a careful interpretation of Huyghens' own words, as expressed in the work above alluded to, he demonstrated beyond doubt that the original ideas of the latter on the spheroidal figure of the earth were of a considerably later date than that usually assigned to them, and that, in his solution of the problem for determining the ratio of the equatoreal and polar axes of the earth, the Dutch geometer had been preceded by Newton, whose method he had, in fact, merely applied to his own theory of gravity. It may be remarked, however, that both these points admit of being satisfactorily established by a reference to Huyghens' work alone, independently of any other source of evidence.

But, in connexion with this subject, there still remains the interesting point to be examined, whether Newton was originally led to conjecture the spheroidal figure of the earth by reflecting on the consequences of the theory of gravitation, or whether he was indebted to some extraneous source for the suggestion of so novel an idea. Humboldt, in a recently published work (*Kosmos*, Band I. s. 172), seems disposed to think that Newton received the first hint on this subject from Cassini's observations of the spheroidal figure of *Jupiter*. This might seem at first sight to be impossible, since it is generally understood that Cassini first announced to the world his discovery of the spheroidal figure of *Jupiter*, in a paper communicated to the Academy of Sciences in the year 1691, that is to say, four years subsequent to the publication of the *Principia*. Humboldt, however, remarks that Cassini's earliest observations of the spheroidal figure of *Jupiter* ascend to an epoch which is at least twenty-one years anterior to the publication of Newton's immortal work. In support of this

statement he refers to a passage in the third volume of Lalande's *Astronomy*, wherein the author asserts that he saw in Maraldi's possession several printed sheets of a fragmentary treatise by Cassini on the spots of the planets, from which it appeared that, at least as early as the year 1666, the Italian astronomer had ascertained the spheroidal figure of *Jupiter*. The illustrious philosopher just referred to has suggested that Newton may have become acquainted with these observations of Cassini by personal intercourse with the scientific men of his day, and thus have been led by analogy to his remarkable conjecture relative to the figure of the earth.*

In the hope of obtaining materials which might throw some light upon this interesting subject, I recently undertook an examination of various historical records, the results of which I have embodied in the following notes. It affords me great pleasure to have this opportunity of expressing my sincere thanks to the President and Council of the Royal Society, who very kindly accorded to me the privilege of examining the early MSS. of the Society while engaged in this inquiry.

Statement of Facts connected with the Early History of the Researches of Astronomers on the Spheroidal Figure of the Planets.

1664, Dec. 14. At a meeting of the Royal Society, Hooke remarked that as everything in nature is liable to change, so the force of gravity might probably vary at the earth's surface. He added that, as it seemed to have an affinity to the magnetic virtue, it might, as in the case of that force, *be more intense at the poles than at the equator*.—(*Journal Book of the Royal Society*, vol. i.)

1666. According to Lalande, the spheroidal figure of *Jupiter* was remarked by Cassini previous to this year.—(*Astronomie*, 3^{ieme} edition, tom. iii. p. 335.)

1667, April 3. It is recorded in the Registers of the Royal Academy of Paris, corresponding to this date, that *the experiment with the pendulum is conformable to the theory of Copernicus*.—(Delambre, *Hist. Ast. Mod.*, tome i., Addit. p. lxiii.)

1671. The *Mésure de la Terre* of Picard was published in this year. Alluding to the standards of measure of different lengths which are derivable from observations of the rate of oscillation of the pendulum, the author thus remarks: — "Ces mésures uni-

* M. Humboldt evidently attaches considerable importance to this point in the history of the inquiry respecting the figure of the earth, for he alludes to it in the same work on two subsequent occasions. In support of his views, he cites a passage of Brewster's *Life of Newton*, wherein the author states, that Cassini's observations of the oblate figure of *Jupiter* had *probably* suggested to Newton the idea of the earth having a similar figure. I am gratified to state, that the distinguished philosopher to whose authority M. Humboldt refers has expressed himself perfectly satisfied with the conclusions at which I have arrived in this communication. I am inclined to think that if the evidence here presented had been accessible to M. Humboldt, he would also have been induced to modify his opinions on the subject.

verselles supposent que la différence des lieux ne cause aucune variation sensible aux pendules. Il est vrai qu'on a fait à Londres à Lyon et à Boulogne en Italie quelques expériences, d'où il semble que l'on pourrait conclure que les pendules doivent être plus courts à mésure que l'on avance vers l'équateur: *conformement à la conjecture qui avait été deja proposée dans l'assemblée, que, supposé le mouvement de la terre, les poids devraient déscendre avec moins de force sous l'équateur que sous les poles.*"—(*Mésure de la Terre*, p. 5.)

1673, April 13. In the *Histoire Céleste* of Lemonnier, consisting of a collection of extracts from the Registers of the Royal Observatory of Paris, which was first published in the year 1741, there is to be found the following observation by Picard, corresponding to this date:—"Je commençai à m'aperçevoir que le disque de *Jupiter* était *un peu ovale*, et que le plus grande diamètre est toujours suivant les bandes."—(*Hist. Cél.* p. 28.)

1678, Feb. 28. At a meeting of the Royal Society, a discussion arose respecting a remark made by M. Gallet, in an account of the transit of *Mercury*, observed by him at Avignon, on the 7th of November, 1677. M. Gallet stated that when the planet was projected on the sun's disc, it appeared to be of an oval figure, the longest diameter being parallel to the equinoctial.* Mr. Henshaw suggested that the appearance might arise from the refraction of an atmosphere about the planet. Mr. Hooke conceived that the body itself of *Mercury* might be of such a figure; and that it might proceed from the velocity of its whirling round upon its axis, he supposing that the axis of its vertiginous or diurnal motion lies north and south, or at right angles with the seeming motion of its parallel to the ;† that a very swift vertiginous motion on that axis made the body of *Mercury* somewhat of the shape of a turnip, or of a solid made by an ellipsis turned round upon its shorter diameter.—(*Journal Book of the Royal Society*, Vol. vi. p. 55.)

March 7. At the meeting held this day, the remark of M. Gallet respecting the oval figure of *Mercury* was further discussed. Hooke's hypothesis was objected to on the ground that, although such an oval figure would be caused by the whirling round of a fluid body, yet it was probable that the body of *Mercury* is solid; and, consequently, the whirling could have no effect upon its figure. To which Mr. Hooke replied, that although it might now be a solid body, yet that at the beginning it might have been fluid enough to receive that shape; and that, though this supposition should not be granted, it would be probable enough that it would readily run into that shape, and make the same appearance; and that it is not

* This is manifestly the spurious phenomenon (termed the *gutta nigra*) which has been observed in modern times on the occasion of transits of the inferior planets. See Baily's paper "On the Annular Eclipse of the Sun of 1836."—*Mem. Ast. Soc.* vol. x.

† This *hiatus* appears in the original MS.: the reader will have no difficulty in supplying the omission.

improbable but that the water here about the earth might do it in some measure by the influence of the diurnal motion, which, compounded with that of the moon, he conceived to be the cause of the tides.*—(*Journal Book*, vol. vi. p. 60.)

1679. Richer's work, containing the account of his observations with the pendulum at Cayenne, was published in this year. It is stated by the author, that the pendulum was found to oscillate more slowly at Cayenne than at Paris.

1686, July 14. In a letter to Halley respecting the claims of Hooke to the discoveries announced in the *Principia*, Newton thus writes:—"There was another thing in Mr. Hooke's letters which he will think I had from him. He told me that my proposed experiment about the descent of falling bodies was not the only way to prove the motion of the earth; and so added the experiment of your pendulum clock at St. Helena as an argument of gravity being lessened at the equator by the diurnal motion. The experiment was new to me, but not the notion; for in that very paper, which I told you was writ sometime above fifteen years ago, and, to the best of my memory, was writ eighteen or nineteen years ago, I calculated the force of ascent at the equator, arising from the earth's diurnal motion, in order to know what would be the diminution of gravity thereby. But yet, to do this business right, is a thing of far greater difficulty than I was aware of.—(*Guard Book of the Royal Society*, N. 1.)

1686, Sept. 3. Newton thus writes to Flamsteed:—"There is another thing said to be observed of *Jupiter* by Cassini long since; namely, that his diameter from pole to pole is shorter than from east to west. If this were certain, it would conduce much to the stating of the reason of the precession of the equinoxes."—(*Biog. Brit.* art. Newton, p. 3229.)

1687, Feb. 2–9. At the meetings of the Royal Society held on these days, Hooke contended that the figure of the earth is that of a spheroid, the shortest axis of which passes through the poles; and he cites, in support of this opinion, the oval figures of the Sun, *Mercury*, and *Jupiter*, as observed by Scheiner, Gallet, and Cassini, respectively.—(*Journal Book*, vol. viii. pp. 127–129.)

1687, April 6. Book iii. of the *Principia* was presented to the Royal Society. In Prop. xviii. there appears the following statement:—"Sic Jovis diameter (consentientibus observationibus Cassinii et Flamstedii) brevior deprehenditur inter polos quam ab oriente in occidentem."

1690. Huyghens' treatise, entitled, *Traité sur la Cause de la Pesanteur*, was first published.† The author states in this work

* These remarkable *aperçus* of Hooke's on the figure of the earth are also to be found in Birch's *History of the Royal Society*, but they seem to have wholly escaped the notice of scientific writers. It may be mentioned, that the words of the original MS. are closely adhered to in the passages here cited.

† This work was originally published in French at Leyden, in the year 1690, while Huyghens was still alive. A Latin edition was afterwards published at Amsterdam in 1728, along with several posthumous works of the author.

that he was originally led to conclude that the figure of the earth is spheroidal by reflecting on the variable rate of the oscillations of the pendulum in different latitudes, as indicated by the observations of Richer at Cayenne. He also mentions, that shortly after he arrived at this conclusion, he was led to investigate the actual ellipticity of the earth upon his own hypothesis of gravity by the perusal of Newton's researches on the same subject, contained in the *Principia*.

Remarks on the foregoing Statement of Facts.

It will be seen that there exists no necessity for appealing to the authority of Lalande in regard to the question of Cassini having observed the spheroidal figure of *Jupiter* previous to the publication of the *Principia*, since Newton expressly alludes to the observations of the Italian astronomer, both in his letter to Flamsteed, above cited, and also in the original edition of the *Principia*. It is manifest, however, that Cassini did not place much reliance upon his early observations of the ellipticity of *Jupiter*, since we find him asserting, as late as the year 1690, that the planet appeared quite round (*Anc. Mem. Acad. des Sciences*, tom. ii. p. 108).

It is worthy of remark that, although Flamsteed is cited by Newton, in the first edition of the *Principia*, as having ascertained by observation that the figure of *Jupiter* is spheroidal, this positive statement is suppressed in the second and third editions. Instead of the words, "consentientibus observationibus Cassinii et Flamstedii," which appear in the original edition (Book iii. Prop. xviii.), the words, "consentientibus astronomorum observationibus," are used in the second and third editions. It might seem, at first sight, as if this alteration had no special reference to Flamsteed, since the name of Cassini is also omitted. We find, however, in the following proposition of both editions, an express mention of the observations of Cassini, but not of those of Flamsteed; whereas, in the original edition, the same proposition contains no allusion to the observations of either of these astronomers. I shall not presume to suggest what may have been the motive which induced Newton thus to suppress the name of Flamsteed in connexion with so interesting a fact as the independent observation of the spheroidal figure of *Jupiter*. I may state, however, that I have carefully examined the *Historia Cœlestis*, and also Baily's *Life of Flamsteed*, but I have not found in them any suspicion of the planet being otherwise than perfectly round. It appears to me that, if Flamsteed had reposed any confidence in his observations, I should have found them recorded in one of the works just cited.

It appears that as early as the year 1667 or 1668, Newton was led to consider the diminution of gravity at the equator arising from the diurnal motion of the earth. As he was then endeavouring to explain the mechanics of the solar system by the principle of gravitation, it can hardly be doubted that the spheroidal figure of

the earth, which is a direct consequence of this diminution, had already presented itself to his mind. It would seem from his letter to Flamsteed, that he had only obtained some vague information respecting Cassini's observation of the spheroidal figure of *Jupiter*, and that he regarded such an observation as affording a valuable confirmation of his own abstract researches on the spheroidal figure of the earth upon which the phenomenon of the precession of the equinoxes (alluded to by him) essentially depends. It is certain, however, from the words used by him in his letter to Halley, "But yet to do this business right is a thing of far greater difficulty than I was aware of,"—that he had not at this time devised the method for computing the ratio of the equatoreal and polar axes which he has given in the *Principia*. The difficulty to which he refers manifestly arose from the complication of the effects of the centrifugal force at the equator with those due to the variation in the intensity of gravity occasioned by the greater distance from the centre of the earth of the particles constituting the redundant matter at the equator.

That Cassini's early observations of the spheroidal figure of *Jupiter* were regarded by Newton as valuable, inasmuch as they served, not to suggest, but to confirm, his own theoretical researches, will further appear evident from the following considerations:—

In 1678, when the question of the spheroidal figure of the earth was discussed at several successive meetings of the *Royal Society*, no allusion was made by Hooke, or any other Fellow of the Society, to Cassini's discovery of the spheroidal figure of *Jupiter*. As this fact would have served to throw much valuable light upon the subject of discussion, we may fairly conclude that it was altogether unknown to any of the individuals who attended these meetings. But if Hooke and his scientific friends in London had not yet obtained any account of Cassini's observations, it is in the highest degree improbable that Newton — who was living in comparative retirement at Cambridge, and whose intercourse with men of science was at this time very limited — should have been better informed on the subject.

It seems probable that the presentation of the first two books of the *Principia* to the *Royal Society* in the spring of 1686, and Halley's active dissemination of a knowledge of the important labours of Newton on the System of the World, may have given rise to much discussion both in England and on the Continent, and that out of this discussion, Cassini's discovery of the spheroidal figure of *Jupiter* (or rather his suspicion of that fact, for it was nothing more) first became known in this country. This view of the subject will appear still more credible when it is borne in mind, that at one of the meetings of the *Royal Society*, held in the spring of 1686, we now find Hooke, for the first time, alluding to the observations of Cassini.

That an important scientific fact closely connected with Newton's researches, might have remained unknown to that geometer

many years subsequent to its discovery, is strikingly illustrated by the circumstance that, at the time of the publication of the second edition of the *Principia*, in 1713, he appears to have been unacquainted with Cassini's comparatively recent communication to the Academy of Sciences, in 1691, respecting the spheroidal figure of *Jupiter;* for, if he had been aware of that announcement, he would assuredly have referred to it in the new edition; whereas, we find merely a vague allusion to the early observations of Cassini. It is in the third edition, which was published in 1726, that Newton for the first time alludes to Cassini's communication to the Academy of Sciences in 1691, the vaguer statement which appeared in the second edition being now altogether suppressed.

It can hardly be doubted, when we take into consideration these various circumstances, that, when Newton first became acquainted with Cassini's early observations of the spheroidal figure of *Jupiter*, he regarded them as valuable merely on account of their affording a confirmation of his own theoretical researches on the spheroidal figure of the earth and the precession of the equinoxes.

About the same time that Newton was led to suspect the variation of gravity at the surface of the earth, or at any rate prior to the year 1671, a suspicion of the same fact was announced at one of the meetings of the Academy of Sciences of Paris. It is worthy of remark, however, that, although Delambre carefully examined the early registers of the Academy, he was unable to discover in them the slightest allusion to the spheroidal figure of the earth.

The earliest distinct announcement of the probability of the earth being somewhat flattened at the poles, as a consequence of its diurnal rotation, is, undoubtedly, due to Hooke. The hints on the subject thrown out by that distinguished philosopher at several successive meetings of the Royal Society in the spring of 1678, afford abundant proofs of the sagacity of their author; but, unfortunately, as in the case of his other speculations on the principle of gravity, he failed to establish his conclusions by a process of rigorous investigation.

It is difficult to ascertain the precise time at which Huyghens was led to suspect that the figure of the earth must be spheroidal. It would appear from the preface to his essay, *On the Cause of Gravity*, that the part of the work which treats of this subject was written after his departure from Paris, and, therefore, subsequently to 1681; and as we further gather from another statement made by him in the body of the same work, that he had been led to suspect the spheroidal figure of the earth *previous* to his perusal of the *Principia*, we may infer that his original speculations on the subject were suggested to his mind on some occasion during the interval comprised between the years 1681 and 1687. His subsequent computation of the terrestrial ellipticity, according to his own ideas of gravity, was suggested by the method given by Newton in the *Principia*.

It may be remarked that the spheroidal figure of the earth was originally suggested to Hooke by the announcement of an ana-

logous fact (which, however, turned out to be unfounded), as characterising one of the other bodies of the planetary system. Huyghens, on the other hand, was led to a similar conclusion, by reflecting upon the variation of gravity at the earth's surface, as indicated by the observations with the pendulum. It is highly probable that Newton's views on the same subject were suggested by purely mechanical considerations.

The following conclusions may be considered as fairly deducible from the facts above stated :—

1. The earliest authentic announcement of the spheroidal figure of *Jupiter* is contained in an observation of the planet by Picard in the year 1673. There are good reasons for supposing that about the same time, or even previously, Cassini was led by his observations of the planet to suspect the same fact; but the Italian astronomer does not seem to have entertained a firm conviction of its existence till about the year 1691. It would appear from the evidence of Newton, that Flamsteed was also one of the early observers of the spheroidal figure of *Jupiter:* this statement, however, is not borne out by an actual examination of the recorded observations of the English astronomer.

2. In the year 1667, or 1668, Newton seems to have been led by his speculations on gravity to consider the effects of the centrifugal force in diminishing the weight of bodies at the equator. It is highly probable that the spheroidal figure of the earth had occurred to his mind on this occasion; but we have no reason to suppose that he was acquainted, at so early a period of his researches, with the method subsequently given by him in the *Principia* for determining the ratio of the equatoreal and polar axes.

3. About the time when Newton first turned his attention to the effects of the centrifugal force at the equator, occasioned by the diurnal rotation of the earth, the probability of a diminution of gravity from the poles to the equator, arising from the same cause, was suggested at one of the meetings of the Royal Academy of Sciences of Paris. It does not appear, however, that any inference was deduced from this fact relative to the spheroidal figure of the earth.

4. In the year 1678, Hooke suggested that the figure of the earth might be spheroidal in consequence of its diurnal motion, but neither on that nor on any future occasion did he assign a method for determining the ratio of the equatoreal and polar axes.

5. Some time during the period comprised between the years 1681 and 1687, Huyghens arrived at the conclusion that the figure of the earth must be spheroidal, but his subsequent determination of the ellipticity, according to his own hypothesis of gravity, was founded on the method given by Newton in the *Principia.*

6. There exist no grounds for supposing that Cassini's observations of the spheroidal figure of *Jupiter* were known in England previous to the year 1686.

New Planet

Discovered July 22 at $11^h\ 45^m$ mean time.

1854.	Regent's Pk. M.T. h m s	R.A. h m s	N.P.D. ° ′ ″
July 22	11 56 55	21 9 50·69	106 20 26·:
..	13 9 29	21 9 48·42	20 45·5
..	13 40 47	21 9 47·72	20 50·2
23	10 46 52	21 9 1·29	106 23 13·7

The Planet shines as a star of 10.9 magnitude.

J. R. Hind.

Mr. Bishop's Observatory,
Regent's Park, July 24.

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A Supplementary Number will shortly be issued, which will contain a few additional Papers, besides the Index and other matters relating to the completion of Vol. XIV. of the Monthly Notices.

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ROYAL ASTRONOMICAL SOCIETY.

VOL. XIV. *Supplemental Notice.* No. 9.

New Planet.

A new planet has been discovered by Mr. Ferguson at the National Observatory, Washington. The following statement respecting the discovery appears in a circular issued by Dr. Gould, the Editor of the *Astronomical Journal:*—

"Mr. Ferguson announces that he discovered, September 1, a new asteroid near *Egeria*. Its place on the 2d, at $13^h\ 30^m$, was R.A. $= 1^h\ 52^m\ 10^s$, Decl. $= 2°\ 57'$ S. It is retrograding 35^s, and moving north $1'\ 40''$ per day. On the 2d, it preceded *Egeria* 24^s, and was nearly as large.

"Mr. Ferguson's observations give,

	Washington M.T.	R.A.	Decl.
	h m s	h m s	° ′ ″
Thirty-first Asteroid, Sept. 2	13 31 8	1 52 10·6	−2 57 4
Egeria	11 27 18	1 52 34·0	−2 57 56

Cambridge, 1854, *September* 5.

The following observation of the planet is due to M. Bruhns, of the Berlin Observatory:—

	Berlin M.T.	R.A.	Decl.
	h m s	° ′ ″	° ′ ″
Sept. 29	14 42 5·6	23 23 31·2	−2 34 20·4

Mr. Hind has received a letter from Dr. Gould, Editor of the *Astronomical Journal*, in which the writer begs to draw the attention of the readers of the *Monthly Notices* to the following statement:—

"As impediments exist to the distribution of the English copies of the *Astronomical Journal* in the same manner as heretofore, I would request those subscribers who are desirous of continuing to receive the Journal, to send in their names once more, either to me at Cambridge or to Messrs. Trübner and Co., 12 Paternoster Row. The Journal can be sent from Cambridge by mail or distributed by the London agent, as may be preferred, the United States' postage being added in the former case to the regular price of subscription.

"A notice to this effect would probably enable me to bring everything into order again. I know you will excuse my request if it seems intrusive, for I do not see any other means of accomplishing the desired end."

Double Stars observed at Madras with the Cerebours Equatoreal.
By Capt. W. S. Jacob.

Synonym.	R.A. h m	N.P.D. ° ′	Position Angle. °	Wt.	No. of Obs.	Mag. Power.	Distance. ″	Wt.	No. of Obs.	Magnitudes. ′	
α Piscium	1 55	87 57	327·73	6	5	365	3·23	4	6	5′–5′	1854·165
			327·97	4′	5	277	3·05	4	6	...	·167
			327·84	5′	5	...	3·25	5	8	...	·170
		Mean result	327·83	16	15		3·18	13	20		1854·167
B.A.C. 1573	4 59	125 41	314·37	4	5	277	2·62	3	6	5–10	1854·167
			315·84	5	6	...	2·54	3′	6	5′–10′	·197
			316·34	4	5	...	2·81	3′	6	5–10′	·208
		Mean result	315·60	13	16		2·66	10	18		1854·191
ζ Orionis	5 33	92 2	151·24	5	5	365	2·23	3′	6	2–5′	1854·162
			151·43	6	5	...	2·14	4	6	...	·165
			151·32	5	6	...	2·21	4	6	...	·167
		Mean result	151·34	16	16		2·19	11	18		1854·165
Δ 23	6 1	138 28	352·63	4	5	277	2·29	3	6	7–7	1854·186
			351·91	5	7	...	2·28	4	6	...	·222
		Mean result	352·23	9	12		2·28	7	12		1854·206
B.A.C.3921,2	11 25	118 27	210·03	7′	6	277	8·58	4′	8	6–6	1854·204
			210·05	7	5	...	8·77	4	6	...	·206
		Mean result	210·04	14	11		8·67	8	14		1854·205
α Centauri	14 30	150 23	279·19	7	6	277	4·14	4′	8	1–1′	1854·234
			278·91	6	6	...	4·19	3′	6	...	·272
		Mean result	279·06	13	12		4·16	8	14		1854·252
α Scorpii	16 20	116 6	274·00	5′	5	365	3·03	3′	6	1′–8′	1854·237
			273·26	4′	5	277	2·93	2′	6		·254
		Mean result	273·67	10	10		2·99	6	12		1854·245
α Herculis	17 8	75 26	118·25	7′	7	365	4·50	3′	6	3′–5′	1854·256
			118·34	7	6	...	4·50	3′	6	...	·259
			118·25	5″	5	...	4·52	3	6	...	·273
		Mean result	118·28	20	18		4·506	10	18		1854·262
70 Ophiuchi	17 58	87 27	113·24	6′	5	365	6·52	3′	6	...	1854·237
			112·84	7′	6	...	6·51	3′	6	...	·240
		Mean result	113·03	14	11		6·51	7	12		1854·239
γ Cor. Aust.	18 56	127 16	355·07	2′	4	277	...	...	...	5′–5′	1854·254
			356·29	5′	5	365	1·74	2′	6	...	·256
			356·53	5′	6	...	1·69	4′	8	...	·259
		Mean result	356·36	13	15		1·71	7	14		1854·257

Extracts of a Letter from Captain Toynbee to Admiral Smyth.

Dated at Sea (Lat. 11° 29′·5, Long. 83° 46′ E.), May 12, 1854.

"Early in March I commenced a series of ☀ — ☾ lunars, knowing that many sailors prefer these to ☉ — ☾ lunars, I wish to show that they are equally useful for rating chronometers. Early in April we were in Table Bay and took no lunars, so that I had to wait until early in May. The observations have been taken between the hours of six and eight P.M., that time suiting me best: the altitudes and distances are all taken by myself, having an officer to note the time by the chronometer. I shall commence by giving you the result of a mistake I made. By an oversight, not having taken ☀ — ☾ lunars before, I confounded the correction for ☉'s parallax with the third correction for refraction, and omitted to apply this third correction to my first series of lunars. I proceed to give you the errors of a chronometer for Greenwich time, found before the third correction was applied to the lunar distances, and then those errors deduced after the distances had been corrected. The remarkable fact is, that the means of these errors agree, so that I had a correct result before the third correction was applied, which in some cases amounted to nearly two miles of distance.

						Third Correction. Not applied.	Third Correction. Applied.
			°			h m s	m s
Mar. 6,	Regulus	E. ☽	71	made Dent	1759 fast of Gr. M.T.	13 0	16 4·5
7,	—		59	—	—	14 7·5	16 51·5
8,	Aldebaran	W. ☽	34	—	—	14 47	12 56
	Regulus	E. ☽	47	—	—	13 50	15 49·5
9,	♄	W. ☽	56½	—	—	15 6	12 42·5
	♂	E. ☽	40¼	—	—	13 18·8	16 17·3
10,	♄	W. ☽	68¼	—	—	15 51·3	12 57·3
	♂	E. ☽	28	—	—	13 0·8	15 40·3
11,	♄	W. ☽	81	—	—	16 56·5	13 38
	Aldebaran	W. ☽	70½	—	—	16 33·8	13 38·8
10) 89						10) 46 31·7	46 35·7
	9th March			—	—	14 39·2	14 39·6

"This is a remarkable proof of the advantage derived from combining an equal number of east and west lunars; and I think it proves that any constant error in observer or instrument would be destroyed. This month (May) I obtained eighteen sets, nine east and nine west of moon: their mean gave, May 6, chronometer fast of Greenwich 22m 38s·2; and fifty-eight days having elapsed, I have a rate of 8s·3 gaining daily. Now I find, that had I only taken the two sets on the 9th of March, and the two sets on the 6th of May, I should have had the same rate, and an original error only 11s less than that by the mean of all the observations."

In a subsequent part of his letter, Captain Toynbee makes the following remark:—"I should like to see the apparent time of the principal stars passing the meridian entered in the *Nautical Almanac*. I am aware this may be found by the difference of their right ascension from that of the sun, but this is not generally known amongst sailors. Besides, the sailor wants, by reference to the table, to find what stars will pass the meridian at such and such a time, but he may have to subtract the sun's right ascension from many stars before he finds the right one."

The planet discovered by Mr. Hind on the 22d of July has been called *Urania*, at the suggestion of Professor De Morgan, who had been invited by Mr. Bishop to select a name for the stranger.

Elements of Urania. By Dr. Oudemans.

Epoch, July 22·0, M. T. Greenwich.

M	.....	298° 13′ 17″·4	
π	.....	26 42 59·3	Mean Equinox, 1854, Jan. 1
Ω	.....	307 57 51·15	
i	.....	1 56 41·7	
φ	.....	8 54 39·2	log e 9·190046
μ	.....	979·715	log a 0·372604

Observations employed in calculating the foregoing elements:—

	Greenwich M.T.	App. R.A.	App. Decl.
	h m s	° ′ ″	° ′ ″
July 22	12 56 21	317 27 14·1	−16 20 40·6
	Leyden M.T.		
Aug. 12	10 47 19	312 20 31·5	−17 21 46·5
Sept. 5	9 23 33	307 46 54·7	−18 6 12·3

The first observation is the mean of three positions taken by Mr. Hind on the night of the discovery of the planet. All the corrections are taken into account. The system of elements is considered by Dr. Oudemans to be very uncertain, since the planet, during the interval comprised between the 22d of July and the 5th of September, had described nearly a great circle.

Discovery of a New Comet (*Comet IV.* 1854). By M. Bruhns.

On the evening of the 12th of September, a new comet was discovered by M. Bruhns, at the Observatory of Berlin, between

the constellations *Draco* and *Camelopardalus*. About $7^h\,45^m$ there was visible in the comet-searcher a very faint nebula, which, when observed with the refractor, exhibited a diameter of several minutes, and by its proper motion was speedily recognised to be a comet. By four comparisons with one of Argelander's stars, M. Bruhns obtained the following position:—

	Berlin M.T.	R.A.	Decl.
	h m s	° ′ ″	° ′ ″
Sept. 12	9 34 48·5	118 38 28·6	+75 5 17·6

The daily motion is,

In R.A. about $+5\frac{1}{2}^\circ$ In Decl. about $-30'$

Elements of Comet IV. 1854. By M. Bruhns.

T ..... 1854, Oct. 27·0722 Berlin M.T.

	° ′ ″	
ϖ	93 21 21·2	Mean Equinox 1855·0
☊	324 43 15·6	
i	40 57 34·7	
Log *q*	9·906626	

Motion direct.

These elements are calculated from the following observations:—

	Berlin M.T.	App. R.A.	App. Decl.
	h m s	° ′ ″	° ′ ″
Sept. 12	9 34 48·5	118 50 28·6	+75 5 17·6
15	10 1 5·3	134 1 38·1	+72 57 40·9
18	8 43 18·9	145 5 39·6	70 5 12·0

The mean observation will be represented thus:—

		″
In Longitude,	Theo.—Obs.	+7·3
Latitude,	—	−3·8

Observations of Comet IV. 1854, *taken at Mr. Bishop's Observatory.*

	Gr. M.T.	R.A.	Decl.
1854.	h m s	h m s	° ′ ″
Sept. 20	8 22 4	10 3 37·14	+67 49 50·9
21	7 50 25	13 18·51	66 41 6·8
24	12 42 44	10 38 59·71	+62 46 52·3

Places of Comet II. of 1854, *as observed at Madras with the Lerebours Equatoreal.* By Captain W. S. Jacob.

1854.	Madras Sid. Time. h m s	R.A. h m s	N.P.D. ° ′ ″	Star compared.	Power used.
April 5	8 14 0 (*a*)	2 42 39·0	74 28 0		113
6	8 7 20	54 22·8	75 27 0	Independent	80
7	7 55 0 (*b*)	3 5 13·8	76 27 6		113
8	8 4 0	15 33·20	77 27 41	B.A.C. 1087	113
10	7 55 31 (*c*)	34 6·35	79 28 7	1174	113
11	8 5 20	42 34·67	80 26 39·6	W. iii. 841	83
12	8 9 37	50 29·27	81 23 6·0	903 & 990	—
13	8 8 53	57 50·70	82 17 31·5	1087 & 1147	—
14	8 22 30	4 4 49·65	83 10 27·0	W. iv. 41	—
15	8 20 50 (*d*)	11 19·02	84 0 40·0	170	—
18	8 45 22	28 44·78	86 19 50·0	608	—
19	8 44 55	4 33 53·74	87 1 44·0	695	—

Assumed Apparent Places of Comparison Stars.

Star.	R.A. h m s	N.P.D. ° ′ ″
B.A.C. 1087	3 22 47·80	77 33 58·0
1174	40 14·80	79 18 37·9
W. iii. 841	43 21·94	80 23 24·5
903	46 49·70	81 15 19·0
990	50 51·25	81 23 44·0
1087	56 0·90	82 12 38·0
1147	3 59 0·60	82 12 23·0
W. iv. 41	4 3 12·80	83 5 47
170	9 14·24	84 1 6
608	28 16·90	86 14 6
695	4 31 53·90	87 4 24

"(*a*) Nucleus bright, in form of a crescent; light = a star 5th mag.; outline of envelope a fine parabola; extremity undefined. From the low altitude and hazy sky on this and the two following days, no star could be found for comparison, and the places are uncertain to about 30″.

"(*b*) Nucleus only, seen through clouds.

"(*c*) Sky clear; bright moonlight; envelope barely visible, nearly 1° long; nucleus more concentrated, reddish, with ill-defined *horns* towards W.

"(*d*) Wind so violent and gusty that the chronometer-beats could not be heard. Observations otherwise good; instrument being perfectly steady."

On the Difference of Longitude between the Observatories of Brussels and Greenwich, as determined by Galvanic Signals. By G. B. Airy, Esq., Astronomer Royal.

This Memoir is divided into six Sections.

Section I. *History and General Arrangements.*

"The increasing importance of galvanic communication in the

operations of Observing Astronomy and the completion of the line of submarine telegraph from the South Foreland to Sandgate, induced me, in the winter of 1851, to solicit the sanction of the Lords Commissioners of the Admiralty for the establishment of a galvanic system at the Royal Observatory of Greenwich, and of wires connecting it with the principal telegraph offices in London. Their Lordships were pleased immediately to grant the necessary authority; and the Directors and Officers of the South-Eastern Railway Company and of the Electric Telegraph Company gave every assistance in their power. The wires were, in consequence, laid in the summer of 1852. Although (in consequence of a change in the relations of the Telegraph Companies) these wires were not used as was at first contemplated, for communication with the Continent; yet their establishment was of great importance, as giving me communication with all parts of Britain, and as enabling me to make the proper arrangements for satisfactory use of galvanic communication by any system of wires. The first determinations of difference of longitude from Greenwich (which, as far as the galvanic operations were concerned, were perfectly satisfactory) were those of Cambridge and Edinburgh, in the summer of 1853; both which were made with these wires.

"In the mean time, the Submarine and European Telegraph Company had laid another system of submarine wires from the South Foreland to Ostend, communicating by the Belgian telegraph wires immediately to Brussels. They had also laid a number of subterraneous wires from their Office in Cornhill to the commencement of the submarine lines at the South Foreland. The subterraneous line passed by the side of the Dover Road across Blackheath, at a distance of less than half a mile from the Royal Observatory. Upon my making application to the Chairman and Directors of the Submarine and European Telegraph Company, permission was immediately given me to connect a wire from the Royal Observatory with one of the Company's wires."

The author then proceeds to describe the way in which this connexion was effected, not only for explanation of the course pursued in the observations detailed in the Memoir, but also for the purpose of showing the liberality with which the Company acceded to his views and the trust which they have placed in him.

"The interest with which M. Arago and other members of the French Académie des Sciences had in 1851 urged the determination of their difference of longitude, and the priority of completion of the submarine telegraph to the French coast, had induced me to contemplate the investigation of our difference of longitude with Paris, as the first use to be made of our connexion with the Submarine Company's wires; and in the summer and autumn of 1853 negotiations for that purpose were in progress with the Bureau des Longitudes. These were interrupted by the illness of M. Arago and other causes. I then considered myself at liberty to commence with the longitude of Brussels; and on my communicating with M.

Quetelet, I found him most anxious to proceed with the enterprise. At the representation of M. Quetelet, and with the sanction of the Belgian Government, a wire was laid from the Telegraph Office in the *Station du Nord* at Brussels to the Observatory, and the good offices of the Telegraph Company were assured for making the wire connexions at the proper hour of the evening.

"A galvanic telegraph-needle was mounted in the Observatory in close proximity to the transit-clock, nearly as in the Greenwich Observatory. The signals to be made were simple deviations of the needle, produced directly by the galvanic current through the long communicating wire; and the observation was to consist merely of careful observation and register of the clock-time of each deviation of the needle.

"It was arranged that the observations should be divided into two series: that in the first series an observer from Brussels (M. Bouvy) should observe both the galvanic signals and the transits for correcting the transit-clock at Greenwich, while an observer from Greenwich (Mr. Dunkin) made the corresponding observations at Brussels; that this series should be continued till at least three evenings' observations, satisfactory in the determination of clock-correction, as well as in the record of the signals, should be obtained; that the observers should then be reversed, and the second series be observed in the same manner. The signals were to occupy one hour in each evening, from 10^h to 11^h Brussels Mean Solar Time ($9^h\ 43^m$ to $10^h\ 43^m$ Greenwich, nearly), each hour being divided into four quarters. The contacts of wires for completing galvanic circuit were to be made at Greenwich and with a Greenwich battery in the first and third quarters, and at Brussels with a Brussels battery in the second and fourth quarters (or *vice versâ*), and between the two sets of observations at each place the poles of the battery were to be reversed.

"It was laid down as indispensable that the persons who observed the signals should not make the contact by which the circuit is completed and the signals are given.

"The following was the routine of an evening's observations of signals in the first series. In the second series everything was similar, except that Brussels commenced instead of Greenwich. By 'warning signals' are meant signals at intervals of 3^s nearly, intended only to be counted; by 'observation signals' are meant signals at intervals of 14^s or 15^s, intended to be accurately observed.

"In the first quarter of hour Greenwich gave four warning signals, as token of readiness. Brussels answered by four warning signals.

"Greenwich gave two warning signals, signifying 'transits,' and then gave warning signals equal in number to the transits of stars observed in the last preceding evening after galvanic signals. If no transits had been observed, the two signals were omitted.

"Greenwich gave observation signals in groups of seven to ten in

number, each group being preceded by a similar number of warning signals.

"In the second quarter of hour Brussels gave signals like those of Greenwich in the first quarter (omitting the four signals).

"In the third quarter of hour Greenwich gave two warning signals for 'transits,' and then gave warning signals equal in number to the transits of stars observed this evening. If no transits were observed, the two were omitted.

"Greenwich gave observation signals in groups, with warning signals as before, the poles of the battery being reversed.

"On the day on which, in the opinion of the Greenwich observers, the operation might properly terminate, Greenwich gave fifteen warning signals.

"In the fourth quarter of hour Brussels gave signals like those of Greenwich in the third quarter."

The transit-clocks were corrected by two distinct methods, which were originally suggested by Professor Challis, in the operations for determining the longitude of Cambridge.

"In method A transits of fundamental stars were used, their right ascensions being taken for the reductions at both observatories from the Greenwich Fundamental Catalogue. The stars employed were not necessarily the same at the two observatories.

"For method B two lists of stars were proposed, one preceding and one following the signals. The stars' places were not supposed to be at all exact, but before using them for correcting the clock the lists of stars observed at the two observatories were compared, and all were rejected which were observed at only one observatory. In this manner the transits of strictly the same stars were compared, and the correctness of their assumed right ascensions was unimportant."

The determinations of the collimation error, level error, and azimuthal error of the transit-instruments were referred to the judgment of the superintendents of the two observatories.

"I think it probable," says the author, "that a course generally similar to that described above will be found convenient in any future operation of similar character. In the method of giving the signals, however, I have at this time (August 1854) made a great improvement. By means of an auxiliary clock, the circuit is automatically completed, and the signal is given at every 15^s, as shown by that clock. The approximate knowledge of the time of signal enables the observer to concentrate all his attention on the observation, and the difference between the rate of the auxiliary clock and the rate of the transit clock (which it is in the power of the superintendent to adjust) causes the signals to occur at different portions of the seconds shown by the transit-clock.

"In circumstances which permit the record of clock-seconds and observations, or signals by punctures produced by a galvanic magnet upon a revolving disk or barrel, or the transmission of transits by a galvanic wire, a simpler method may be employed."

The section closes with a brief journal of the operations.

Section II. *Comparison of Observations of the Galvanic Signals by different Observers.*

"As it was intended by the interchange of observers to eliminate the errors arising from personal equation, as well of signal observation as of transit observation, no particular effort was made to obtain a comparison of the various modes of observation. No efficient comparison of the two real observers (Messrs. Bouvy and Dunkin) could have been made without a most inconvenient extension of the operation."

A few comparisons were, however, incidentally made between the observations of Mr. Bouvy on the one hand, and those of two of the Greenwich Assistants on the other. The author concludes this section with a synopsis of the mean results, and the extreme ranges on each side.

Section III. *Comparisons of the Recorded Clock-Times of Galvanic Signals observed at Brussels and Greenwich, which are not accompanied with Astronomical Observations.*

"The signals which were not accompanied by observations of transits are useless for the determination of difference of longitude. They are, however, perfectly available for determining the time occupied by the passage of the galvanic current from Greenwich to Brussels, or *vice versâ*, as will appear from the following explanation of the method of treating them.

"In the first series, the signals of the first quarter of each hour were given exclusively by Greenwich contact near the Greenwich battery. Let t be the time occupied by the passage of the current: then, during the first quarter, the times read from the Greenwich clock will not be increased from this cause, but the times read from the Brussels clock will be increased by t. The reading of the Brussels clock is (in consequence of its eastern longitude) usually greater than that of the Greenwich clock. Suppose that the excess as unaffected by time of transmission ought to be found $= E_1$, but that as affected by time of transmission it appears (on comparison of observations) to be e_1, then the comparisons of the observations of the first quarter of an hour give this equation,

$$E_1 + t = e_1$$

where E_1 and t are yet unknown.

"In the second quarter of each hour the signals were given exclusively by Brussels contact near the Brussels battery. In this case the Brussels clock readings were not increased, but the Greenwich clock readings were increased by t, and, therefore, the excess of the Brussels clock readings was diminished by t. Hence these comparisons give

$$E_2 - t = e_2$$

"Similarly the third quarter gives

$$E_3 + t = e_3$$

and the fourth quarter,

$$E_4 - t = e_4$$

"If the two clocks had no relative rate, the four numbers, E_1, E_2, E_3, E_4, would be equal. As it is certain, however, that there was a relative rate, the only supposition that we can venture to make is that it was uniform during the hour. We may also in most cases assume that the mean of the signals in each quarter of hour corresponded (with sufficient approximation) to the middle of the quarter of hour. Let the relative rate for one quarter of hour be r, then the equations become,

$$\begin{aligned} E_1 \quad\quad + t &= e_1 \\ E_1 + \quad r - t &= e_2 \\ E_1 + 2r + t &= e_3 \\ E_1 + 3r - t &= e_4 \end{aligned}$$

"The easiest way of treating these equations is the following:

"Subtract the double of the second equation from the sum of the first and third:

$$4t = e_1 + e_3 - 2e_2, \text{ or } t = \frac{e_1 + e_3 - 2e_2}{4}.$$

"Subtract the sum of the second and fourth equations from the double of the third:

$$4t = 2e_3 - e_2 - e_4, \text{ or } t = \frac{2e_3 - e_2 - e_4}{4}.$$

"The mean of these two determinations may probably be adopted as preferable to either of them alone.

"In the second series, when the signals in the first and third quarters were given from Brussels, the process is the same, but the sign of t must be changed."

The section concludes with the results of the individual day's observations.

Section IV. *Authorities for the Instrumental Errors and Clock Errors on the Days which are available for Determination of Difference of Longitude.*

This section contains an account of the methods used for the determining the coefficients of instrumental and clock correction; and also so much of the numerical values of those coefficients as will enable the reader to judge of the stability of the instruments and the general trustworthiness of the results.

Section V. *Comparison of the Sidereal Times of Galvanic Signals observed at Brussels and Greenwich, on the Days which are available for the Determination of Difference of Longitude.*

"The observed transits were corrected numerically for the effects of the instrumental errors of which the clock

in the last section. The clock-errors were obtained by comparing the corrected transits with the tabular places of the stars specified in the last section; and these clock-errors, duly reduced to the time of signal observation, were applied to the recorded clock-times of signals. The difference of these at the two observatories gives the apparent Difference of Longitude. The results for Difference of Longitude in the separate quarters of hour are treated in the same manner as the simple clock comparisons in Section III., in order to obtain from them the time occupied by the galvanic current in passing between Brussels and Greenwich. For the Difference of Longitude (still subject to the effects of personal equation) it is necessary to take the mean of the four separate results."

The author then gives the numbers obtained on each day by the two independent methods, A and B.

Section VI. *Abstract of the Results for the Time occupied by the Passage of the Galvanic Current and for the Difference of Longitude.*

The definitive result for the time occupied by the Galvanic Current in passing between the two observatories is found to be $0^s{\cdot}109$. This evaluation is based upon 2616 observations.

"The result, $0^s{\cdot}109$, may arise from the sum of two quantities: first, the time really occupied by the passage of the galvanic pulse; secondly, the comparative slowness of perception of the observer at the distant station, arising from the more languid motion of the needle. But having been fully able to appreciate the attention with which the observers watched the needle, and the intentness with which their energies were directed to seize its first movements, —remarking also that, in consequence of the admirable insulation of the telegraph wires, the difference in the intensities of the current at the near and distant stations is very small,—I am disposed to think that the number $0^s{\cdot}109$ represents almost purely the time employed by the transit of the galvanic current.

"The telegraphic distance between Brussels and Greenwich is about 270 miles. If the velocity of the current were uniform, the value obtained from these experiments would be little more than 2500 miles per second, or not more than $\frac{1}{4}$ of that obtained by several American investigators. It is, however, to be remarked, that from Greenwich to London and thence to Ostend, the whole of the line is subterraneous or subaqueous; and that unpublished experiments by Mr. Latimer Clark (which, by the kind permission of that gentleman, I have myself witnessed) show that the velocity in such wires is not greater than 800 or 1000 miles per second. It appears, therefore, extremely probable that the retardation which we have discovered belongs almost entirely to the subterranean and submarine portion of the line; and that the retardation between Ostend and Brussels is practically insensible."

The final results for the Difference of Longitude, as determined by the two methods A and B, are the following:

	Method A.	Method B.
	m s	m s
Mean of first series.....	17 29·256	17 29·340
second series....	17 28·538	17 28·476
Mean of the two series ..	17 28·897	17 28·908

These determinations rest upon 1104 signals. The author concludes with the following remarks:—

"The last result, or $17^m\ 28^s{\cdot}9$, is indisputably the best that can be given at the present time for the Difference of Longitude of the two Observatories. The difference, however, of the results given by the two series, $0^s{\cdot}791$, is somewhat striking. If it be attributed entirely to personal equation, it implies, either that Mr. Bouvy registers signals earlier by $0^s{\cdot}4$ than Mr. Dunkin would do in the same circumstances, their registers of transits of stars being identical; or that Mr. Bouvy registers transits later by $0^s{\cdot}4$ than Mr. Dunkin would do, their registers of signals being identical; or that Mr. Bouvy registers signals somewhat earlier and transits somewhat later, the sum of the two discordances amounting to $0^s{\cdot}4$. Neither of these explanations is impossible, and the second is supported by the remark which M. Quetelet has communicated to me, that Mr. Bouvy registers transits later by $0^s{\cdot}3$ or $0^s{\cdot}4$ than M. Quetelet does, while there is no sensible difference between their modes of observing signals. The first suggested explanation is not supported by the observations recorded in Section II.

"The only remaining explanation of the difference of results of the two series, which appears possible, is, an error in the evaluation in one or both series of the instrumental errors. I have endeavoured to give all the means for decision on this point, and must leave it to the judgment of the reader.

"When the American* method of recording transits by galvanic punctures upon a revolving plate or barrel shall be more extensively introduced into Europe, I think it probable that operations equal in value to that described in this paper may be effected in a shorter time. But, even with the signals of the telegraph needle, repeated for a few days, the accidental errors of signal observation may be considered as practically eliminated, and those of transit observation will also sensibly vanish. The only remaining sources of error are, the personal equations and the inaccuracy of instrumental adjustments: the former may be eliminated by interchange of observers, but for the latter only the most careful attention to the principle and the practice of the methods employed will suffice."

* The author here appends the following foot-note:—"I use this term as implying that the method in question was practically introduced and extensively applied in America before it was employed in any other country. I know not by whom or where the method was first proposed."

List of Instruments for sale, in the Observatory of the late R. Snow, Esq. at Ashurst.

A Transit Instrument of $3\frac{1}{2}$ feet focal length, and $2\frac{3}{4}$ inches' aperture, with an axis of 18 inches from shoulder to shoulder. By Mr. Simms, Fleet Street.

An Equatoreal by Mr. Simms, of 5 feet focal length, and 3·9 inches' clear aperture. The hour-circle is 2 feet, and the declination-circle 18 inches in diameter. This instrument is made to follow a star by a clock-work motion, slightly modified from Fraunhofer's plan. It has also a spider-line micrometer by Simms, the highest power of eye-piece being 500.

A Transit Clock by Molyneux, with the usual dead-beat escapement and mercurial pendulum.

A Journeyman Clock by Molyneux.

A 20-inch portable Transit Instrument by Troughton.

A 45-inch Achromatic Telescope by Dollond. A mahogany three-legged stand; an equatoreal block; various eye-pieces; a parallel spider-line micrometer; and sundries, accompany this instrument.

A portable Night Telescope and Comet-seeker by Simms, with an iron equatoreal-stand by Robinson.

An Eight-day Chronometer by Molyneux, showing sidereal time.

A Daniell's Hygrometer by Newman.

A Mountain Barometer by Newman.

A Mountain Barometer by Troughton.

Two Thermometers.

A pair of small Globes.

ERRATA.

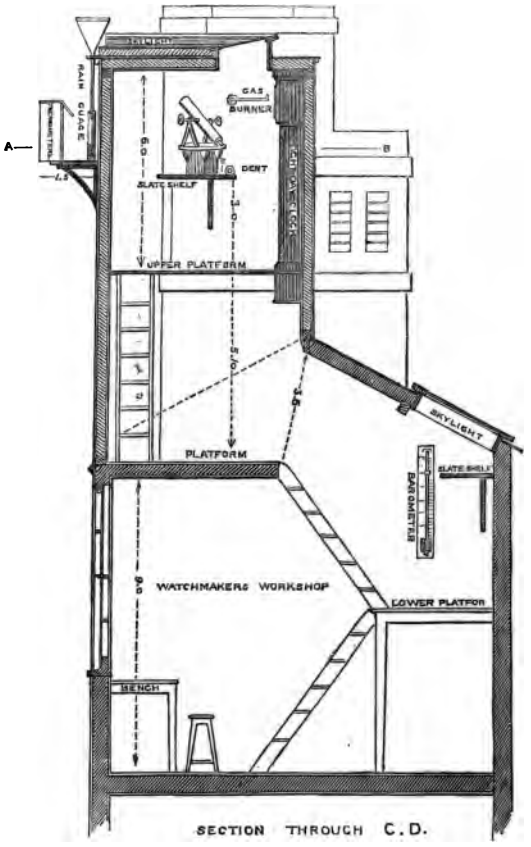
Vol. XIV., page 18, line 3 from top, *for* and the western, *read* on the western.

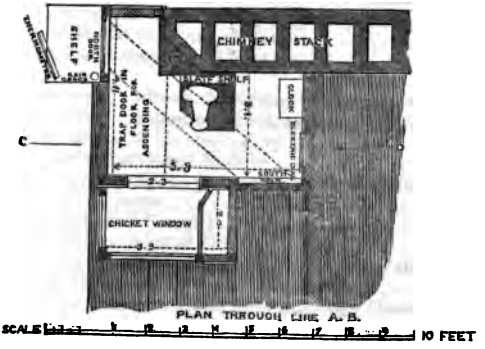
— — 80, — 18 from top, *for* 1853, *read* 1835.

— — 163. It ought to have been stated that the date of Mr. Hippisley's observation of *Saturn* is February 12, 1854. See page 192.

— — 193. After line 7 from top, read thus:—Were balloted for and duly elected Fellows of the Society.

Drawing of the Miniature Observatory of Messrs. H. Ellis and Son, Exeter, of which a description is given by Dr. Lee at page 44.





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